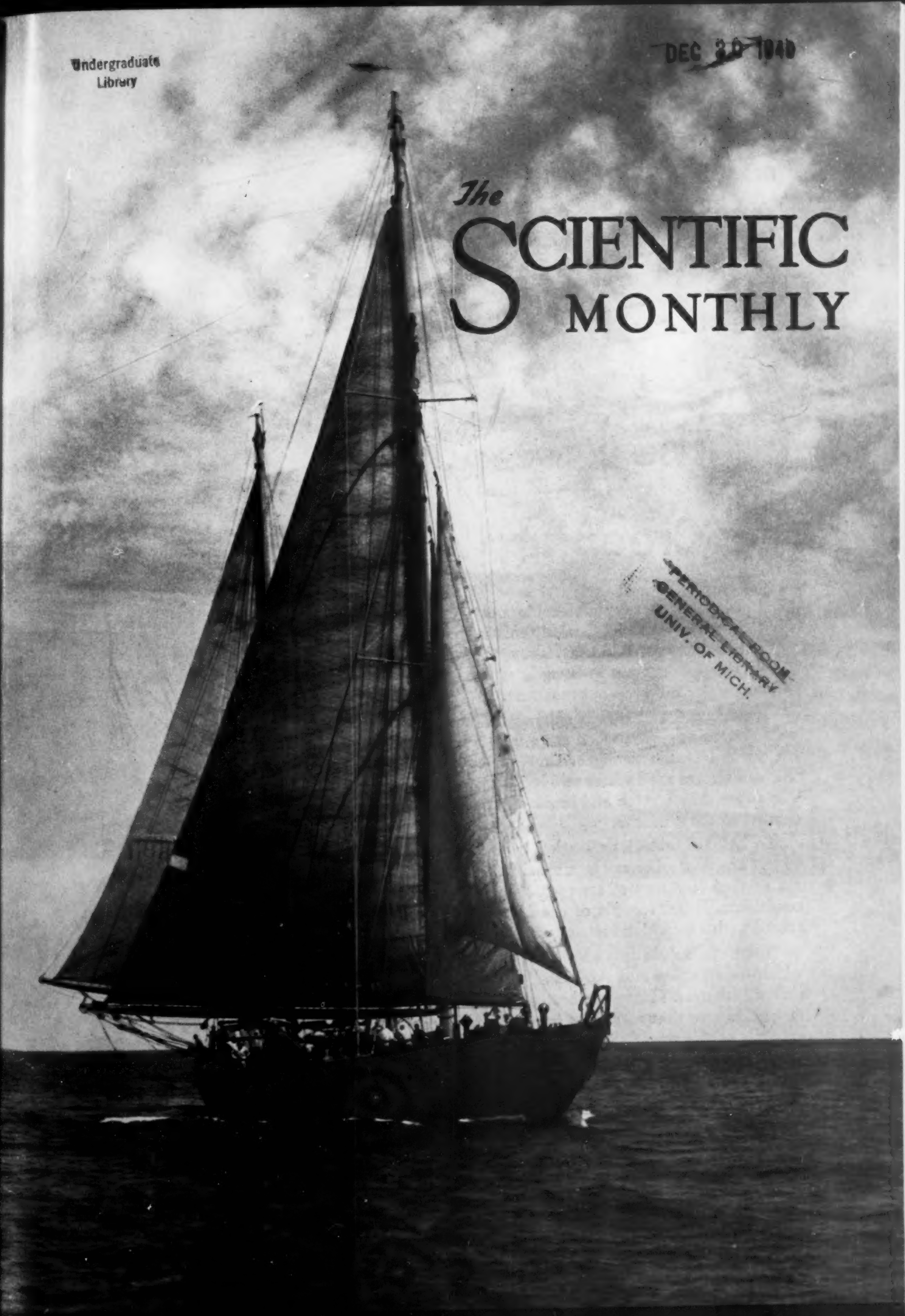
The SCIENTIFIC MONTHLY



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ITINERARY

SECOND (1948) INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

Science Illustrated New York City	January 6–20	American Osteopathic Convention St. Louis, Missouri	July 11–17
General Foods Corporation Hoboken, New Jersey	February 3–24	Auburn Camera Club Auburn, Alabama	July 25–August 1
Brown University Providence, Rhode Island	March 7–26	Pennsylvania State Museum Harrisburg, Pennsylvania	September 5–26
Gamma Sigma Epsilon University of Florida, Gainesville	April 15–May 1	University of Utah Salt Lake City	October 5–26
Sigma Pi Sigma University of Oklahoma, Norman	June	Royal Photographic Society London, England	December 1–31

A special exhibit made up of sixty duplicates of entries in the complete Salon is available.

THE SCIENTIFIC MONTHLY

JANUARY 1949

THE ULTIMATE STANDARD OF LENGTH

WILLIAM F. MEGGERS

Dr. Meggers has been Chief of the Spectroscopy Section of the National Bureau of Standards since 1920. He received his Ph.D. from Johns Hopkins University in 1917. Figure 4 in the article below won first prize for Dr. Meggers in the black-and-white section of last year's Photography-in-Science Salon, an annual competition sponsored by the Smithsonian Institution and THE SCIENTIFIC MONTHLY.

IT NOW appears that the ultimate standard of length has been found in a wave length of radiation emitted by mercury 198, an isotope transmuted from gold by neutron bombardment. It will be shown that the homogeneity, reproducibility, and convenience of this standard cannot be surpassed by any other. It is, therefore, inevitably the ultimate standard of length, basic for the definition of all other units, including the meter.

The meter, as everyone knows, was designed about 1790 to represent one ten-millionth of the earth's quadrant. In 1827, some natural philosophers, meeting in Paris, speculated that the meter could not be reproduced if the form or size of the earth were changed by collision with a comet. This inspired Sir Humphry Davy, the English chemist, to propose as a natural standard, independent of terrestrial form, the diameter of a capillary tube of glass in which water would rise to a height exactly equal to the tube's diameter. Recognizing the defects of this standard, Jacques Babinet, a French natural philosopher, suggested that a wave length of light in a vacuum would be a better one. The same suggestion was later made independently by German, Dutch, and British scientists, but the first practical results must be credited to two Americans, A. A. Michelson and E. W. Morley, who, in 1887, outlined "A method of making the wavelength of sodium light the actual and practical standard of length." Their method involved the use of the optical interferometer, which they devised for their celebrated experiments on the relative motion of the earth and ether; it consisted of the measurement of a length by counting an equivalent number of interference fringes.

Although the meter was originally intended to be the 10,000,000th part of the earth's meridional quadrant through Paris, a platinum end standard (*Mètre des Archives*) of approximately this length was accepted as basic for the metric system until superseded in 1889 by the present International Prototype Meter, which is defined by the distance at the temperature of melting ice between the centers of two lines traced on the platinum-iridium bar deposited at the International Bureau of Weights and Measures. Within the limited accuracy of comparison the present Prototype is identical with the original meter, but its legal definition does not refer to any natural standards or to physical constants.

In 1889 Michelson and Morley described in detail a method of measuring the meter in light waves and stated that the brilliant green mercury line would in all probability be the wave to be used as the ultimate standard of length. When Michelson applied his interferometer to a study of the homogeneity of spectral lines, he discovered that atomic radiations, in general, were not strictly

monochromatic. In particular, he found that the green mercury line was one of the most complex in nature, and that the red light of cadmium was most nearly homogeneous.

Michelson, in 1892, went to the International Bureau of Weights and Measures and made the first accurate determination of the relation between the meter and the wave length of cadmium red radiation. This experiment was repeated in 1905 by three French scientists, and their value, 6,438.4696 A, or 10^{-10} meter, was adopted in 1907 as the *primary standard of wave length* and definition of the angstrom unit. During the next forty years the most precise measurements of length were made with this standard.

During World War I there was great anxiety and fear that a bomb might accidentally destroy the world's Prototype Meter. The best way to insure the meter against accident or change is to define it in terms of an indestructible but accurately and easily reproducible wave length. In 1927 the Bureau of Standards recommended that the International Conference on Weights and Measures do this, but a conservative Conference defined, *in a provisional manner,* 1 meter equals 1,553,164.13 wave lengths of red radiation from cadmium and explained that it was not a question of giving a true relation between the meter and the wave length, but only a metric value of the latter which could be modified by future experiments. In the next double decade the meter-wavelength experiment was repeated seven more times, thus making nine determinations in all. A summary, recently published by H. Barrell, of the (British) National Physical Laboratory, is shown in Table 1. The average deviation of one of these values from the arithmetical mean of all of them is one part in seven million, which is truly remarkable considering that each ruled line on the meter is ten to twelve wave lengths wide. Notice that the final average of all is identical with the value measured in 1905. This may be regarded as proof that in a period of forty-eight years the meter did not change its length beyond the limit of accuracy of these measurements. However, metal end gauges, susceptible of greater accuracy of wavelength measurement than ruled scales, have been found generally to change with time, and there is no a priori reason to believe that any material standard of length is strictly immutable. It may be assumed, on the other hand, that a wave length of monochromatic light, produced and observed under specified conditions, represents a permanent, reproducible, unchanging, and sharply definable unit of length; all

TABLE 1

VALUES OF THE WAVE LENGTH OF THE CADMIUM RED LINE IN TERMS OF THE INTERNATIONAL METRE (UNIT = 1×10^{-10}m)

DATE OF DETERMINATION	OBSERVERS	ORIGINAL VALUES	CORRECTED AND ADJUSTED VALUES IN NORMAL AIR	DIFFERENCES FROM MEAN	
				10^{-10}m	Parts per 10^6
1892–93	Michelson and Benoît (B.I.P.M.)	6,438.4722	6,438.4691	− 0.0005	− 0.08
1905–06	Benoît, Fabry and Perot (B.I.P.M.)	6,438.4696	6,438.4703	+ 0.0007	+ 0.11
1927	Watanabe and Imaizumi (Tokyo)	6,438.4685	6,438.4682	− 0.0014	− 0.22
1933	Sears and Barrell (N.P.L.)	6,438.4711	6,438.4713	+ 0.0017	+ 0.26
1933	Kösters and Lampe (P.T.R.)	6,438.4672	6,438.4689	− 0.0007	− 0.11
1934–35	Sears and Barrell (N.P.L.)	6,438.4709	6,438.4709	+ 0.0013	+ 0.20
1934–35	Kösters and Lampe (P.T.R.)	6,438.4685	6,438.4690	− 0.0006	− 0.09
1937	Kösters and Lampe (P.T.R.)	6,438.4700	6,438.4700	+ 0.0004	+ 0.06
1940	Romanova, Varlich, Kartashev and Batarchukova (Leningrad)	6,438.4677	6,438.4687	− 0.0009	− 0.14
		Mean	6,438.4696	± 0.0009	± 0.14

experience supports this assumption. Although a wave of green light is only 1/50,000 inch in length, it can be reproduced within 1/100,000,000 of its length, and length measurements with light waves can be made with this accuracy. Even if the ruled lines on meters could be bisected to 1/100 of their width, length measurements with homogeneous light waves are capable of tenfold greater accuracy.

In order to attain the maximum accuracy of length measurements with light waves, it is necessary to employ the most homogeneous or monochromatic waves that can be found. Sixty years ago it was generally assumed that all spectral lines were monochromatic and invariable. In 1892 Professor Michelson devised an interference method of testing this assumption and found that most of the lines examined were complex; that is, instead of being single they consisted of two or more close components with unequal intensities. Michelson proved that the effective wave length of such a group of lines would depend on the phase relations and relative intensities of the components when applied to length measurements. He found the green mercury line the most complex, and the red cadmium line the most homogeneous, of any that he tested. Consequently, he discarded the green line of mercury as an ultimate standard and determined the number of waves of red radiation from cadmium vapor equivalent to one meter.

Michelson's discovery, in 1892, of spectral line complexity led to intensive investigation of this phenomenon, but an acceptable explanation of it was delayed for nearly forty years. Even though chemical isotopes were discovered in 1913, it was necessary to await the development of the quantum theory of atomic spectra before the complex structure of spectral lines could positively be ascribed to atomic nuclei. The actual phenomena are somewhat complicated, but they are simply illustrated by the mercury green line whose structure was first accurately observed and interpreted in 1931. Theory and experiment agree that this line has sixteen components as shown in the left half of Figure 1.

Natural mercury consists of a mixture of seven isotopes with mass numbers (relative to oxygen = 16) 196, 198, 199, 200, 201, 202, and 204. All are characterized by the well-known spectrum of mercury, consisting, in the visible range, of two close yellow lines (5,791 and 5,770 A), a bright green line (5,461 A), a strong blue line (4,358 A), and a violet line (4,047 A). To these (and other) mercury lines each isotope contributes one or more components of which not any are exactly coincident. The component displacements have two different causes, isotope shift and nuclear spin. Mercury isotopes with even mass numbers (but no nuclear spin) contribute single components shifted in accordance with mass as shown in Figure 1. The two isotopes with odd mass numbers, 199 and 201, have nuclear spins (of 1/2 and 3/2 units, respectively) which interact with the valence or optical electrons to produce close groups or clusters of lines called hyperfine multiplets. Thus the green line of natural mercury receives three components from Hg^{199} and eight from Hg^{201}, which, added to five from even-mass isotopes, totals sixteen.

The objectionable complexity of mercury lines could be eliminated if one even isotope could be separated from the rest, but until recently it has not been practicable to isolate or concentrate an isotope of natural mercury in sufficient

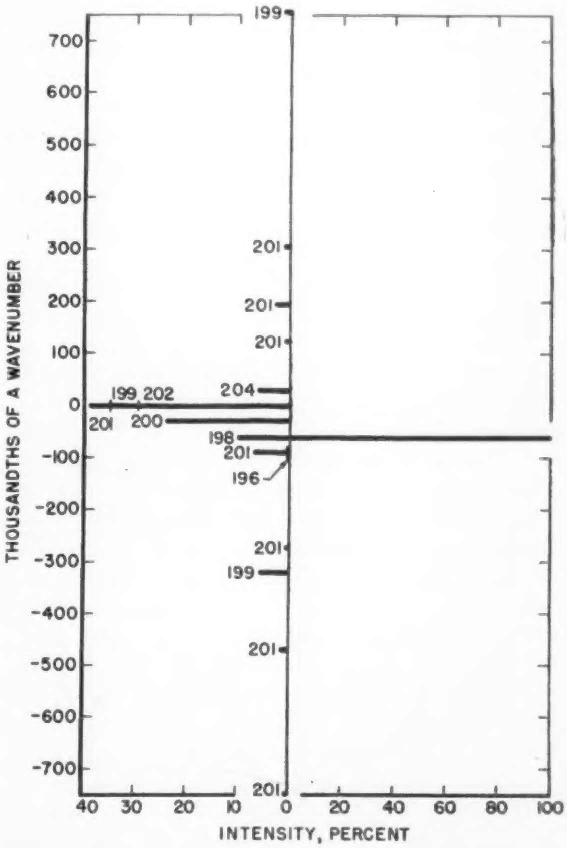

Fig. 1. (*Left*) The thick horizontal lines represent the 16 components of green radiation from natural mercury; their lengths are proportional to intensities (or isotopic abundances), and their isotopic origins are indicated by mass numbers. (*Right*) Green radiation from mercury 198 has only one component.

quantity to make any lamps. This most desirable end has now been attained, however, by transmuting gold Au^{197} into mercury Hg^{198}, thus reversing the alchemist's age-old experiment for the simple reason that mercury 198 as the ultimate standard of length is worth infinitely more than gold. The nuclear reaction that transmutes gold into mercury was first reported in 1934 from the University of Rome by E. Fermi *et al.*, who used a mixture of beryllium powder and radon as a neutron source to bombard gold. Neutrons that attach themselves to nuclei of gold atoms produce a highly radioactive isotope of gold which decays rapidly (half-life, 2.7 days) and becomes a stable isotope of mercury. Before 1940 such experiments yielded only infinitesimal amounts of transmuted elements, which could not be seen, weighed, or detected except by radioactive effects. Late that year L. W. Alvarez, of the University of California, boldly suggested that neutrons from a cyclotron might transmute sufficient gold into mercury to be detected spectroscopically. Exposure of one ounce of gold to neutrons for one month yielded enough mercury to produce a tiny electrodeless lamp that shone for about five minutes, during which the first interference spectrogram was made, thus demonstrating that the green line of mercury could be emitted entirely free from complex structure. This is shown diagrammatically in the right half of Figure 1.

Recognizing the importance of the experiment reported by J. H. Wiens and L. W. Alvarez, and wishing to increase the production of Hg^{198} so that one or more durable lamps could be made, the National Bureau of Standards purchased forty ounces of proof gold and requested the University of California to bombard it with neutrons for one or more years. Unfortunately, World War II interfered with this project, and only submicroscopic quantities of artificial mercury were made. The prospects were very discouraging until, near the end of the war, there were rumors of a secret source of neutrons thousands of times more effective than the largest cyclotron. In 1945 the National Bureau of Standards' gold was transferred from California to Tennessee. A year later the Bureau distilled from this gold about sixty milligrams of mercury, which was tested with the interferometer and mass spectrometer, and found to be highly pure Hg^{198}. Some of this Hg^{198} has been used in the preparation of several types of lamps. These are being studied to determine which will be best for length measurements.

The ultimate in simplicity of lamp construction has been found in an electrodeless discharge tube,

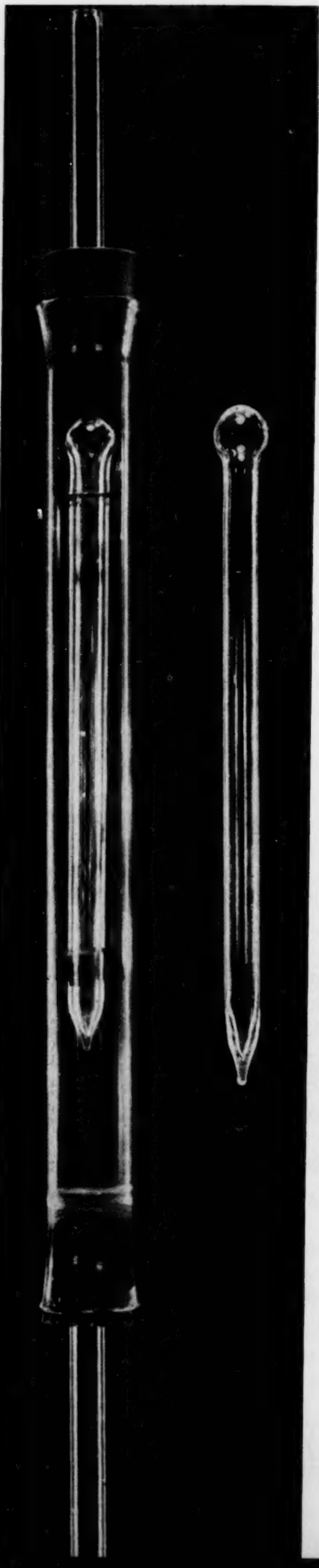

FIG. 2. Mercury lamps, either with or without water cooling, emit light when excited by high-frequency radio waves. A gentle stream of cold water around the lamp insures that the lamp is at a constant low temperature and that it will emit sharp lines without any self-reversal. (Lamps shown are about ¾ natural size.)

made by sealing small amounts of gas or vapor in evacuated tubes of glass or quartz (Fig. 2). When such tubes are held in a high-frequency electrostatic field, the inclosed gas or vapor emits its characteristic atomic radiations. By this method intense light emission can be obtained from mercury at extremely small vapor density and low temperature, which are necessary conditions for sharp lines. Several lamps of this type have been made, each containing a few milligrams of natural mercury or of artificial Hg^{198}. Green light from one of the lamps containing natural mercury was selected with colored glass filters and collimated to illuminate a Fabry-Perot interferometer, which consists simply of two silvered flat planes adjusted accurately parallel as shown in Figure 3. Multiple reflections occur between the planes, and when the transmitted rays are collected by a lens they form at the principal focus a system of circular interference fringes. Such a set of fringes was photographed for natural mercury; the photograph was bisected, and one half is reproduced in Figure 4. The lamp containing natural mercury was next replaced by one containing artificial mercury 198; the same interferometer was illuminated by filtered green light, and the fringes were photographed. One half of the interference pattern of the green light from Hg^{198} is reproduced in Figure 4 juxtaposed to that from natural $Hg^{196, 198, 199, 200, 201, 202, 204}$. Is there any doubt about which is which?

No matter how sharp and single-valued the Hg^{198} fringes may be, they are of no value whatever for length measurements until the lengths of the Hg^{198} light waves themselves have been accurately measured. This is being done by comparing them with the primary standard of wave length, 6,438.4696 A, provisionally adopted as the metric value of the red radiation from Michelson's type of cadmium lamp. Because of the relative coarseness of ruled-meter lines compared with light waves it is possible to measure one light wave in terms of another ten times more accurately than either can be measured relative to the ruled meter.

Comparisons of wave lengths by interferometer methods are among the most beautiful experiments in physical optics; in simplicity and precision they are outstanding among physical measurements. The experimental arrangement is shown pictorially in Figure 5 and diagrammatically in Figure 6. A mercury 198 lamp is imaged inside a cadmium lamp (or vice versa), and light from both lamps simultaneously illuminates an interferometer. The transmitted light is collected by an achromatic lens that images interference patterns on the slit of a spectrograph, which in turn forms a spectrum (by prismatic dispersion) and focuses monochromatic slit images with interference patterns superposed but without the confusion of overlapping. A portion of such an interference spectrogram is reproduced in Figure 7.

The interferometer itself is extremely simple; as stated above, it consists of two perfectly flat glass or quartz plates separated by a certain distance and adjusted accurately parallel. The adjacent faces of such a pair of interferometer plates are coated with thin films of silver or other metal to reflect 80–95 percent of the incident light. When such an interferometer is illuminated by monochromatic radiation and viewed from the other side, a system of perfectly circular interference fringes is seen in the transmitted light appearing to come from infinity. Bright areas result from constructive interference, and dark areas from destructive interference, of the successive reflections and transmissions of light waves. Of particular interest is the illumination at the center of each pattern; if the center exhibits maximum brightness it means that the number of light waves in the double distance (to and fro) between the interferometer plates is an integer, or whole number, because the successive components are all in phase to interfere constructively. If the center is dark it means that the number of waves in the double distance between the plates is an integer plus a fraction which is 1/2 for maximum darkness occurring when the crest of one wave coincides with the trough of another; that is, they are 1/2 wave length out of phase. In general, the fraction will have a value between zero and unity because successive fringes represent a change of one wave length in retardation between the plates. With highly homogeneous waves this fraction is determinable to one one-thousandth of a wave, and herein lies the unique advantage of measuring lengths with light waves; the scale division is ten thousand times finer than the lines ruled on a meter bar.

In terms of any particular light wave the double distance (to and fro) between two parallel interferometer plates is the product of the wave length and the number of waves, which, in general, as just stated, consists of an integer and a fraction. This number of waves in the double distance is called the *retardation*, or the *path difference*, or the *order of interference*. Assuming that the double distance between the interferometer plates is the same for all wave lengths, it can be expressed for each as the product of wave length and appropriate order of interference. The ratio of two wave lengths is, therefore, equal to the inverse ratio of

their orders of interference. Thus, the wave length of the green wave of mercury is determined from that of the red wave of cadmium by multiplying the latter by its order of interference and dividing this product by the order of interference of the former.

In principle, the orders of interference, whatever their value, may be determined by starting with the interferometer plates in contact and then counting fringes which flow out as the plates are separated. This was the procedure actually used by Michelson when he determined the number of cadmium waves in a meter, but to avoid the tedious labor and risk of blunder in counting 1,553,164 waves he counted only about 1,212 contained in 0.39 millimeter and then doubled this number eight times in succession to reach a decimeter, finally comparing this wave-measured decimeter with the meter by displacing the former ten times its own length.

Now, *by using the green and yellow lines of* Hg^{198}, *it is possible to determine orders of interference or measure lengths without counting any fringes at all.* The only requirements are measurements of a few interference-ring diameters, accurate values of the wave lengths, and an approximate value of the distance between the interferometer plates.

The fractional part of the order of interference is derived directly from measurements of ring diameters. It is seen (Figs. 4, 7) that the intervals between successive rings are not constant; the separations decrease with increasing ring number counting from the center. But theory and measurement agree that these rings are a quadratic function; that is, the squared values of successive ring diameters differ by a constant. Because the squared values of these ring diameters are a linear function of ring number, the fractional order at the center of the pattern is simply obtained by dividing the square of the first-ring diameter by the constant difference between squares. Obviously, the unit of length employed in measuring ring diameters is immaterial because the unit cancels out in the quotient.

Assuming that the fractional orders of interference have been thus measured for the green radiation 5,461 A and for the two yellow radiations 5,770 A and 5,791 A, the next step is to determine the whole number or integral order of interference for each of these radiations. Starting with a scaled or calipered approximation of the distance between the interferometer plates, double that distance is divided by 5,791 A to get the nearest whole number of waves. To this trial number is added the measured fractional order, and this sum is then multiplied by the wave length to give a double distance that includes the correct fraction but possibly an erroneous integer. If the trial number is correct, division of the double distance by the wave length 5,770 A will yield a corresponding whole number and fraction, and this computed fraction will agree with the measured fraction for this wave length. If these computed and measured fractions disagree, the divergence shows how large an error exists in the trial whole number, because the rates of change of wave numbers are inversely proportional to the wave lengths. This yellow pair of mercury radiations differs in wave length only 21 A, which is 1/275 of a wave. If the order of interference for 5,791 A is reduced by exactly one wave the order of interference for 5,770 A will be reduced by one plus 1/275 wave (because it is 1/275 shorter), and any given configuration of their interference patterns cannot recur except at intervals of 275 waves. Therefore, dividing the difference between the computed and measured fractions by 1/275 indicates at once how many waves to add (or subtract) from the first trial whole number. Then the corrected double distance is divided by the wave length 5,461 A of the green wave to check the correction and get its integral order of interference (without counting any fringes). In terms of green mercury waves the double distance is this integral order plus the

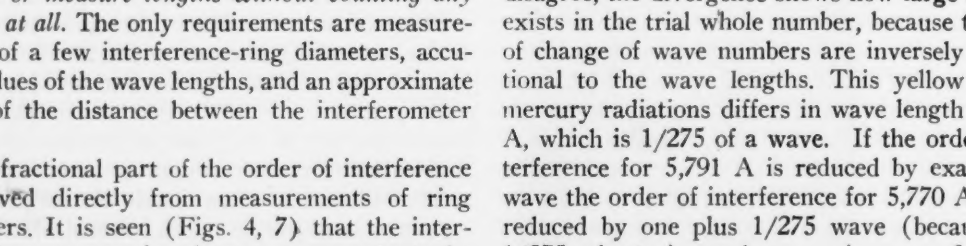

Fig. 3. Light from one point of an extended *light source* is traced through a Fabry-Perot *interferometer* to its image on a *slit*, or screen. This image will be either light or dark, according as the retardation of successive reflections and transmissions is an even or odd multiple of half wave lengths. The symmetry of this condition about the optical axis forms circular fringes.

measured fraction. If the interferometer plates are in optical contact with the end faces of a plane-parallel end gauge, the gauge length will be half of the observed order of interference. Indeed, this is the method actually used since World War I for the absolute measurement of all precision end gauges, "Johansson blocks," and the like, except that mercury lines could not be employed. Heretofore, such measurements with cadmium lines have been restricted to lengths of the order of four inches or ten centimeters, but with Hg^{198} waves it will be possible to measure directly more than ten inches or one-quarter meter.

The advantages of measuring lengths with three radiations of Hg^{198} may be summarized as follows: The yellow pair is happily heuristic for the order of interference since coincidences can recur only at intervals of 275 waves. Any distance known within half of this interval can then be measured to one one-thousandth of a green wave without counting any fringes. Coincidence rates and measured fractional orders lead uniquely to the correct integral orders. Theoretically, with Hg^{198} waves, orders of interference exceeding a million waves can be applied to length measurements. Since the integral order is determinable without error, and the fractional part can be measured to one one-thousandth, uncertainties in length measurements may be reduced to one part in a billion, or thousand million.

Certainly, for purposes of length measurement, the waves of mercury 198 are an improvement over natural mercury; they are also an improvement over waves characteristic of cadmium atoms. Now one may ask the question "Why is a wave length of mercury 198 the *ultimate standard of length?*"

The most nearly ideal wave-length standard must satisfy certain conditions as to monochromaticity, reproducibility, intensity, and convenience.

At low pressures and moderate electrical excitation the monochromaticity of atomic radiations varies as the square root of the atomic mass divided by the absolute temperature. The residual fuzziness is fully explained by the random motions of the radiating particles: those moving toward the detector appear to emit slightly shorter waves and those moving away appear to emit slightly longer waves. (This is an example of the well-known Doppler principle.) Naturally these atomic motions are least for heavy particles at low temperatures. Since mercury atoms are nearly twice as heavy as cadmium atoms, and radiate strongly at less than half the absolute temperature, mercury waves will be less than half as fuzzy as cadmium waves, other things being equal.

Cadmium consists of a mixture of eight isotopes, and most of its lines exhibit hyperfine structure. Although the isotopic structure of the red radiation from cadmium has never been resolved, it is very likely present. Natural mercury consists of seven isotopes, but this isotopic complexity has been removed in the manufacture of mercury 198

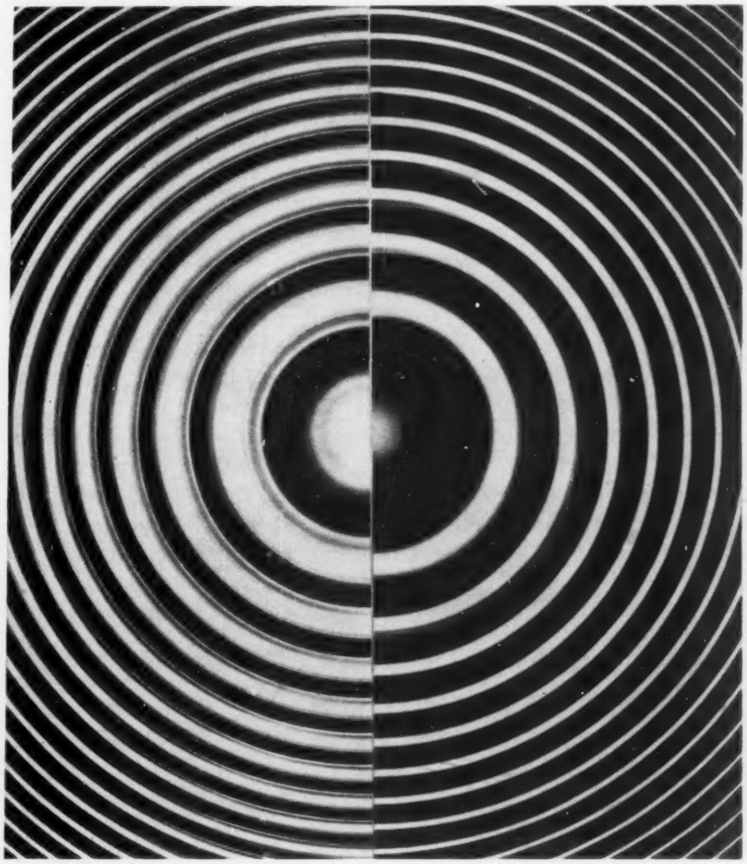

Fig. 4. Circular interference fringes formed by a Fabry-Perot interferometer illuminated with green light from lamps containing (*left*) seven isotopes of natural mercury and (*right*) a single isotope, mercury 198, made by transmuting gold. Faint eccentric fringes on the right are caused by reflections from the last, unsilvered, slightly inclined interferometer surface.

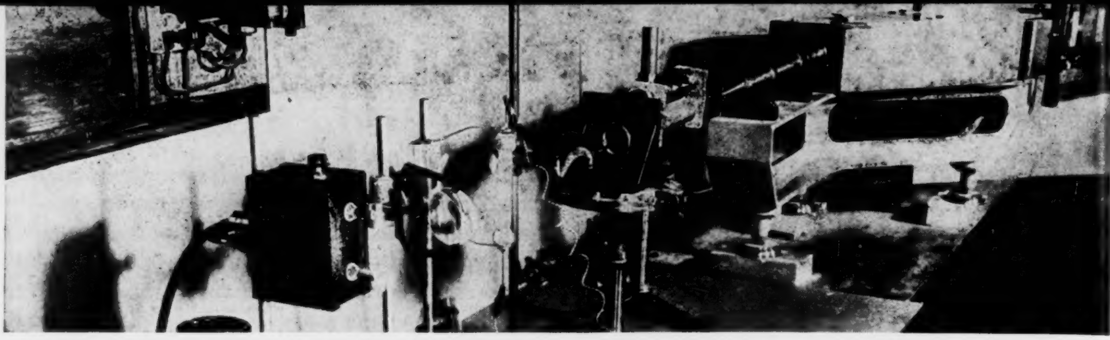

Fig. 5. An optical train for measuring wave lengths emitted by mercury 198 relative to the cadmium standard. The mercury lamp is imaged inside the cadmium lamp so that light from both sources passes simultaneously through a Fabry-Perot interferometer and into a prism spectrograph.

from gold 197. Being a single isotope, the mercury 198 radiations cannot exhibit isotope shifts, and because the atomic nucleus has an even mass number and no detectable angular momentum there can be no hyperfine structure in its spectral lines.

Although cadmium and mercury are divalent chemical analogues, and therefore exhibit relatively simple and similar atomic spectra, whatever differences exist are almost invariably in favor of mercury. For example, the brightest line in the cadmium spectrum occurs in the blue-green (5,086 A), whereas the mercuric analogue is in the green (5,461 A), almost exactly coincident with the wave length for which the normal human eye is most sensitive. The red wave of cadmium (6,438 A) is intrinsically only one-tenth as energetic as the brightest line (5,086 A), and it is further handicapped by the fact that the eye is only one-seventh as sensitive for red as for green. Consequently, for the visual adjustment of interferometers the green line of mercury is seventy times as intense as the red line of cadmium. The mercuric analogue of the cadmium red line (6,438 A) is a yellow line (5,791 A) which is always accompanied by another yellow line of slightly shorter wave length (5,770 A) but nearly equal intensity. This yellow pair of mercury lines produces interference coincidences at intervals of 275 waves and therefore facilitates (without counting any fringes) the determination, from a first approximation, of the uniquely correct order of interference; it has no equally useful counterpart in the spectrum of cadmium or of any heavier element.

Mercury has the lowest freezing point (–39° C) of any metallic element, but even at that point it has a vapor pressure adequate for the excitation of its spectrum by high-frequency electric fields. At room temperature its vapor pressure is about one micron (of Hg), thus insuring the reproducibility of wave lengths emitted by mercury lamps at low or moderate temperatures because pressure displacements and all other effects associated with high vapor density will be inappreciable.

Cadmium melts at 321° C, and to insure enough vapor to produce a spectrum the Michelson lamp must be enclosed in a furnace kept at a temperature between 300° and 320° C. This high temperature adds fuzziness to the waves, and heat convection and radiation from the furnace may disturb the adjustment of the interferometer or change the wave length by altering the air density. Owing to the inconvenience of operating the standard cadmium lamp in a furnace, that source has rarely been used except for measuring the meter and for the determination of some secondary standards of wave length.

Mercury vapor is a pure monatomic gas which with moderate excitation emits a relatively simple spectrum characteristic of neutral atoms without any background of band spectra due to molecules or molecular compounds. The electronic structure and electron-binding forces are such that the spectral lines of mercury are distributed from ultraviolet to infrared. Two ultraviolet lines (1,850 A and 2,537 A) are the most intense; they produce ultraviolet burns (erythema) but are wholly ab-

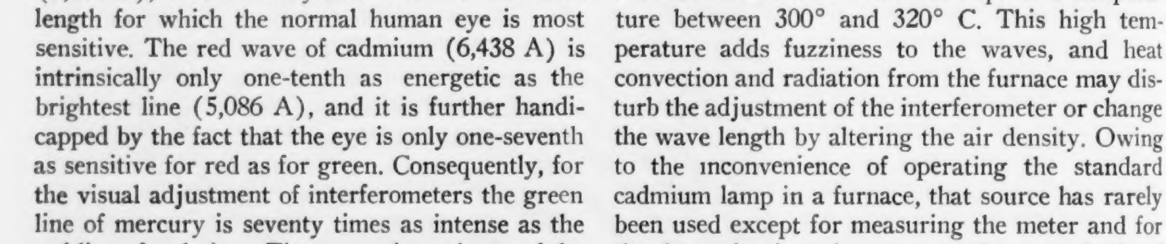

Fig. 6. Schematic diagram of optical train for measuring relative wave lengths, or for measuring lengths in terms of wave lengths. Twice the distance between the interferometer plates is obtained from measurements of the diameter of circular interference fringes. See Figure 7.

sorbed by ordinary glass. The third strongest line is the green one (5,461 A), to be recommended as the ultimate standard of length. With moderate excitation of an electrodeless lamp, interference patterns of this line can be photographed in a few seconds.

A small prism spectrograph suffices for the separation of yellow and green interference patterns when making length measurements, and if the green line alone is desired for counting fringes or for testing optical flats it can be isolated by using appropriate and convenient color filters. Thus the mercury waves of shorter length than green can all be absorbed by a solution of potassium dichromate or by a sheet of shade-yellow glass, and the two yellow lines can be removed by a solution of didymium nitrate or a sheet of didymium glass.

Mercury is the only heavy stable element that has an appreciable vapor pressure at low temperatures, and therefore it is unique among all elements in radiating, at low pressure and temperature, a relatively simple spectrum of intense and exceedingly sharp lines, provided that isotopic structure is eliminated. The green line of mercury, rejected by Michelson fifty-six years ago on account of complex structure, has finally been freed of its seven-isotope curse, and now stands alone as the most nearly ideal standard wave length that can ever be obtained from any atoms, natural or artificial. Coupled with the fact that adequate quantities of pure Hg^{198} can now be made by neutron bombardment of gold in chain-reacting piles, the unique properties of Hg^{198} force the conclusion that a progressive scientific world will soon adopt the wave length of green radiation (5,461 A) from Hg^{198} as the ultimate standard of length.

Since it is the width and character of the ruled lines themselves that limit the accuracy of meter–wavelength intercomparisons, there is hardly any point to measuring the wave length of Hg^{198} green light relative to the meter. This wave length can readily be measured relative to that of cadmium red light ten times more accurately than either can be measured relative to the meter. Adoption of the present provisional relation between the meter and cadmium red wave as exact, and subsequent substitution of Hg^{198} green for cadmium, appear to be the logical and expeditious approach to the ultimate standard of length.

In conclusion, I wish to emphasize that I am not trying to abolish the meter; on the contrary I am anxious to perpetuate it by giving it a scientific definition that will make it more accurately reproducible. The meter is here to stay because it

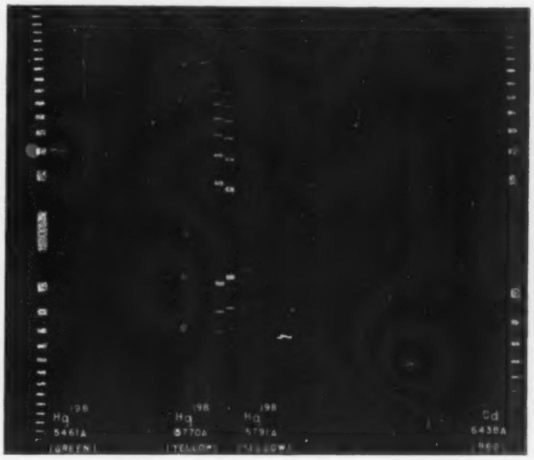

Fig. 7. Fabry-Perot patterns for the green and two yellow radiations of mercury 198 and the red radiation of cadmium, photographed simultaneously by means of a prism spectrograph.

is a very convenient and useful instrument for calibrating ruled scales with which most length measurements are made. It is highly arbitrary and unscientific, however, to define the primary standard of length as a distance between two relatively coarse, irregular lines on a metal-alloy bar, especially since there is no a priori reason for believing that such a material standard will be immutable for all time, or will forever survive all possible accidents and world catastrophes.

Everyone knows there has been a steady demand for increased accuracy of length measurement, both in science and in industry, but the present meter cannot satisfy all the requirements. The spectacular demonstration of interference methods of measuring with light waves by Michelson in 1892 was soon followed by the first set of metric gauge blocks produced by Johansson in 1896. These contributions set new standards of accuracy for the twentieth century, and the most precise measurements of lengths have been made, and will continue to be made, with light waves. It appears that the last possible improvements in this direction can now be made (1) by recognizing the wave length of green light from Hg^{198} as the *Ultimate Standard of Length*, and (2) by perpetuating a constant and more accurately reproducible meter through its definition in terms of the Ultimate Standard of Length *in vacuo*.

BIOLOGICAL PROBLEMS OF THE OCEAN

DANIEL MERRIMAN

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OCEANOGRAPHY in its modern sense—including as it does the physical, chemical, and biological study of ". . . the sea and all that is therein"—is a young science. If we are to date its birth (and this is of little more than academic interest), the three-and-a-half year, 69,000-mile expedition of H. M. S. *Challenger* three quarters of a century ago holds first claim. This cruise, resulting in widespread popular interest, a wealth of oceanographic data, and a magnificent series of publications, gave great impetus to research on the seas.

It is well to remember that although the chief motive of the *Challenger* expedition was biological—the question of life at the depths of the ocean—this was really the first time that scientists interested in the physics, chemistry, and biology of the sea worked together on problems of mutual interest. I spent a good share of last summer in England reading the original longhand, unpublished diaries and notes of some of the men on that expedition: Sir C. Wyville Thomson, Sir John Murray, J. J. Wild, J. Y. Buchanan, Dr. von Willemöes-Suhm and others. If there is any one thing these diaries show (apart from the breadth and capacity of the men themselves), it is the increasing understanding, as the voyage progressed, of the complete interdependence of the observations by chemists, physicists, and biologists in the solution of problems of the ocean. There is now, as then, the absolute necessity that the workers in different branches of oceanography should be in close touch so that they may profit from one another's advances.

A century ago, at the time of the founding of the AAAS, marine biologists were concerned mainly with the distribution of the known forms and the search for, and description of, new species. Some conception of working conditions one hundred years ago may be gleaned from an article in the *Athenaeum* (London) of July 5, 1873, which compares the equipment on the *Challenger* (then on the first year of her expedition) with that of the *Rattlesnake,* which was on surveying work in 1846. The *Rattlesnake* had on board as naturalist Mr. Macgillivray, and as surgeon no less distinguished a naturalist than Mr. Huxley. At Rio de Janeiro they determined to try their luck, and Mr. Macgillivray wrote:

> I had looked forward with eager anticipation to the result of the first dredging of the voyage. *None of the ship's boats could be spared,* so I hired one pulled by four negro slaves, who, although strong active fellows, had great objections to straining their backs at the oar when the dredge was down. *No sieve having been supplied,* we were obliged to sift the contents of the dredge through our hands,—a tedious and superficial mode of examination. Still some fine specimens of a curious flat sea-urchin (*Encope marginata*), and a few shells, encouraged us to persevere. Two days after, Mr. Huxley and myself set to work in Botafago Bay, provided with a *wire-gauze meat-cover,* and a curious machine for cleaning rice; these answered capitally as substitutes for sieves, and enabled us, by a thorough examination of the contents of the dredge, to detect about forty-five species of mollusca and radiata, some of which were new to science.

The writer in the *Athenaeum* for 1873 concludes his comparison of conditions on the *Rattlesnake* and the *Challenger* by the following somewhat acid remark: "The Lords of the Admiralty have, we are glad to know, learned since that time to render it unnecessary for naturalists sent out by them to borrow meat-covers and machines for cleaning rice."

I

The *Challenger* expedition stimulated so much interest and brought back so many new species that marine biologists in the last quarter of the nineteenth century concentrated their efforts chiefly on description. Before long, however, the rate at which new species were discovered began to decrease, and by the turn of the twentieth century attention was focused more on faunistic and life-history studies. About this time the dread of depletion in certain major fisheries loomed large, particularly in the North Sea, which is fished so

intensively by its bordering countries. Accordingly, in Europe there was formed the International Council for the Exploration of the Sea, and here in this country the United States Bureau of Fisheries produced a most distinguished record of research.

Johan Hjort, eminent Norwegian oceanographer, writing in 1907 on the question of the practical use of scientific fisheries investigations, compares agriculture with the fishing industry and sums up the ultimate goal of the fishery biologist of that time:

> . . . in the history of agriculture in every country there has existed a period when all progress consisted in breaking up fresh land, a time when there was far more unutilised soil than there were human beings to till it. This is the period of expansive cultivation. But we also know that, with increasing population, there came in most of the old civilized lands a time when the question was, how to get the fullest advantage out of the fields in use—how to obtain from high farming the most beneficial and economical results. So, too, we find in the fishing industry times when new fishing-grounds are discovered, while again another generation may be occupied with the problem of how to deal most profitably with the old grounds that are known so well.[1]

There were many causes for optimism in this aim of rational utilization. As but one example, the tool of age determination by scales and otoliths was now available; it therefore appeared possible not only to trace the variations in the ages of a stock of fish as a particular fishery developed—a task which would be for the fishing industry what population statistics are for insurance—but also to solve the problem of at what age it is most profitable to catch the fish, both from the point of view of the return to the fisherman and the preservation of the stock for the future. Two fundamental questions were uppermost in the minds of the members of the fishing industry, and it was up to the biologist to answer them:

1. The causes of the great fluctuations in annual yield of many prominent fisheries.
2. The age-old argument as to whether or not the supply of fish was falling off as a consequence of overfishing.

With regard to the first, fluctuations in the fisheries have from time immemorial been of enormous economic importance; in the Middle Ages, for instance, the appearance of herring schools was the deciding factor in the economy of whole provinces —towns sprang up or vanished with equal rapidity as schools of herring came or disappeared; in more recent times certain Scandinavian cod fisheries have ranged in yield between 3 and 23 million fish within the short space of three years.[2] To understand these fluctuations in abundance, it is necessary to know something of the reproductive habits of fishes[3]—quite apart from the causes of change in migratory pattern. In general, there is an inverse relationship between the number of eggs a species lays and the amount of parental care given to those eggs and the developing young—the greater the number of eggs the less the amount of care and vice versa. With fish that lay few or an intermediate number of eggs and give varying degrees of parental care, the mortality rates of the developing young tend to be fairly constant; hence we know in a general way that a certain number of spawners will produce a predictable number of young, and it is therefore possible to control the size of the population. Here it is obviously necessary to protect the adult mature stock, through closed seasons, length limits, etc., so that an adequate recruitment of young is assured. With fish which produce huge numbers of eggs, however, (and accordingly offer them no protection), frequently there are tremendous fluctuations from one year to another. Such fish produce from 50,000 to several million eggs per individual, and most of them scatter their floating eggs in the water to develop as best they may. Many of the great fisheries are based on species of this reproductive type— cod, haddock, mackerel, pilchard (or sardine), bluefish, striped bass (or rockfish). These fish show the phenomenon of dominant year classes; i.e., the production of so many fish in one year that this age group dominates the whole population for a series of years. The classic example of a dominant year class is the Norwegian herring, where the young born in 1904 were so numerous that they were a major element in the catch from 1907 to 1921 and virtually supported the fishery for fifteen years. It is clear, then, that in fisheries with large numbers of eggs the fluctuations in abundance are likely to be due more to the environment than to the size of the adult stock. In short, the number of spawners is not as important as the precise condition of the water in which the eggs and larvae undergo their development. To get good survival there apparently has to be an optimal combination of such factors as temperature, drift, salinity, and of the nutrients that ultimately are responsible for the production of floating or planktonic organisms, which in turn form the food of the newly hatched fish. As an example of the order of magnitude of survival, study of the mackerel[4] shows that the mortality in 1932 from the fertilized egg to a two-inch stage was 99.9996 percent; i.e., the survival was roughly 1–10 fish per million newly spawned eggs. Hence, a fluctuation in sur-

vival of several ten thousandths of a percent may be the difference between average production and the production of a dominant or weak year class.

More adults do not necessarily mean more young. Indeed, dominant year classes have a peculiar predilection for turning up when the adult stock is at its lowest level. In the case of many commercially important fish there is no conclusive evidence that an increase in stock will produce more young. We understand so little of the population mechanics of the stocks of fish which produce large numbers of eggs that we cannot predict with certainty what will happen if the catch is decreased and the number of spawners is increased. This is true so long as the stocks remain somewhere between an extremely high level (where competition between individuals results in great mortality and perhaps in predation on other desirable species) and an extremely low level (where there are an insufficient number of spawners). The size of the stock of most commercially fished species probably lies between these extremes. There is, therefore, no guarantee that more adults mean more young; this is so because the environment is so important in the development of the eggs. If one grants the foregoing reasoning, then one of the main jobs confronting the fishery biologist is to discover for each species the environmental conditions that produce good and bad survivals so that he is able to make predictions of economic value to the fishing industry.[5] Here the fullest exchange of information between the physical, chemical, and biological oceanographer is essential to the solution of the problem. As Fleming has pointed out, the lack of physical boundaries increases the complexity of such biological problems of the ocean.

II

The two questions that faced marine biologists in the early part of this century—causes of fluctuations and the overfishing problem—are still very much in the foreground. Partial answers have been given, but progress over nearly fifty years has been halting and confined to isolated cases. There is still much room for argument and disagreement. There are many reasons why this is so:

1. The fishery biologist is called into the picture only when a particular stock gives cause for alarm; i.e., when it has reached a low level. Seldom does he have the opportunity to study the virgin stock and then to measure the effect of man's intervention on the population.

2. Most of the studies have lacked the necessary continuity. Perhaps a biologist is asked to study a particular species which is at a low level through some natural fluctuation; several years later the population regains some of its stature, and another species gives cause for alarm, so the biologist is taken off the first (just when he's starting to grasp the problem) and moved to the second. Our man power has been used most inefficiently in this respect.

3. There is an acute shortage of trained personnel. The fact is that we know very little about the populations of living forms in the sea. The major feature of fisheries work—the fact which should above all determine the character of the methods and the theory—is that it is a population problem, aligned with the work of Lotka, Volterra, Verhulst, Pearl, Elton, and others on other populations.[6] For a full understanding of these problems, broad training through the graduate level is an essential. Furthermore, the necessity of field observation, the drudgery of the collection of the basic raw data, and the disadvantages of long-term studies in a time when competition for advancement is strong and when there are less arduous fields of research that offer sure and relatively swift publication—all these matters are not calculated to attract young men who have invested many years and much money in their own education.

I have devoted considerable space to biological problems of the ocean dealing with the fisheries for two reasons: first, as we have seen, because historically they deserve prominence; and, second, because they are of direct practical importance. Before turning to biological problems of the ocean which have less obvious practical application, let me mention one more matter dealing with the resources of the sea. In the last quarter of the nineteenth century the attention of biologists was drawn to the deep and open ocean; then, as we have seen, through the need for study of the coastal and bank fisheries, the attention of marine biologists became focused on the margins of the oceans. It is, in my own opinion, long since time that intensive biological study of the high seas by the United States was undertaken. Granted, we are taking steps in the proper direction, but we have a long way to go before we catch up with the prewar knowledge and ability of the Japanese in this regard.[7] Much has been said and written of late about the alarming rate of increase of the human population of the world and the scarcity of food.[8] In this connection the newspaper accounts of the AAAS Centennial symposia attributed a Cassandra-like quality to the remarks of a number of the

scientists gathered for the occasion. It seems inevitable, even taking into full account the possible advances of science, that the oceans will be called upon to produce more food. Clearly, if there is to be any substantial increase, it will have to come from the high seas; intensive research will be needed to produce it and to enable us to fish the available stocks at the optimal level.

III

What are other biological problems of the ocean? They are so many that to attempt to pick out more than a few main fields of endeavor would be ludicrous. Here let me follow some suggestions that come from discussion among British investigators, particularly H. W. Harvey and F. S. Russell of the Marine Biological Laboratory at Plymouth.[9]

1. Half a century ago and later, much attention was given to the description of new species; *individual* specimens were described in some detail. More recently, work on land animals has tended toward the study of *samples* of animals of a single species and their variations, with considerable advance in our understanding of evolutionary processes. It is high time that the study of trends in variation from one area to another was undertaken more intensively on marine animals. This involves the fields of systematics, genetics, geographical distribution, anatomy, etc. And in passing let me state that there is still crying need for good taxonomic work, descriptive anatomy, and embryology, to say nothing of the problems that still await the experimental embryologist who wishes to work on marine animals.

2. Turning to another branch of biology, the physiologist has for some decades conducted highly profitable researches on shallow-water marine forms. But thus far very little has been done on oceanic animals; their respiration, excretion, osmoregulation, coloration, luminescence, digestion, respiratory pigments, and a host of other matters deserve close scrutiny. The problem of the availability of deep-sea forms is by no means insurmountable at such a station as Bermuda.

3. For anyone interested in the study of behavior, the organisms of the ocean offer tremendous possibilities. There is still much to be done in the analysis of the causes of the vertical migration of planktonic animals in deep water; moreover, new tools are now available to make such analysis open to broader attack. As Russell has pointed out, we know very little about the swarming of animals in deep waters, or about their feeding habits. Some of the most fascinating problems are posed by the relationships between two or more species in the same community, whether they be planktonic, benthonic, or nektonic. Similarly, we have barely scratched the surface in understanding the interrelationships between the different animals in different communities.

4. In the study of the life histories of marine animals there is still an incredible amount to be done both in the shallow waters and more especially with oceanic forms. The life histories of relatively few commercial fishes are adequately known, and the invertebrate bottom fauna badly needs study. Surely it is a scandalous state of affairs that with many of the open-sea pelagic game and commercial fishes practically no studies have been made on the age at sexual maturity—that we are literally unable to state at what age or length these fish first spawn, an essential to intelligent thinking on problems of rational utilization.

5. Finally, let me mention briefly some of the problems associated with studies of the fertility of the ocean. It was stressed in this symposium how physical conditions in any area of the sea fluctuate from one period to another and over long periods. Such changes affect the populations of the sea; there are fluctuations in the abundance of different species and in the general fertility. Sometimes, however, great changes in fertility occur that do not appear to coincide directly with any notable change in physical characteristics. Work in the English Channel over many years has shown close positive correlation between the phosphate maximum, the plankton population, and the number of young fish spawned and surviving during the summer; more phosphate or nutrient resulted in greater phytoplankton production, which in turn apparently supported a greater animal population. Harvey believes that there is a reasonable chance that water masses may in the future be labeled in terms of their potential fertility. But there is a long way to go in this direction. When we come to assess potential fertility we shall need to know much more than we do now about the balance between the plants and the grazing animals, and much more, too, about all the chemical factors affecting plant production. We need the answers to these and a host of other questions about the fertility of the seas.

To get some of these answers we shall need the closest exchange between workers in all branches of oceanography. We shall need to provide opportunity for the free and inquiring minds—the best in the field—to go where they will. Carefully planned research is essential, but I think we should bear in

mind that it can be dangerous to plan too much. No one can foresee what new discovery will change our outlook, or what practical result will come from investigation in pure science. Let us not fall into the trap of directed and applied research to the extent that our over-all advance toward the solution of the biological problems of the ocean might suffer.

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Airplane view of Bock Island, Marshall Islands, Ralik Chain, Rongerik Atoll. (Photo by Leonard P. Schultz, Smithsonian Institution.)

POPULATION AND FOOD SUPPLY: THE CURRENT SCARE

M. K. BENNETT

An expert in international trade in foodstuffs, prices, production, and standards of living, Dr. Bennett (Ph.D., Stanford, 1927), executive director of the Food Research Institute at Stanford University since 1942, here sounds a rather different note than the one we have lately been hearing from other experts in similar fields.

THE so-called "race between population and food supply" has again come forward as an absorbing topic of discussion, in popular and professional circles alike. Discussants mostly present a deeply pessimistic view of the future. At a press conference on May 18, 1948, Sir John Boyd Orr, retiring director of the Food and Agriculture Organization of the United Nations, is reported to have "pleaded with the correspondents to help awaken the people to the fact that in the race between population and food, population was winning —'and we do not know how to stop it.' "[1] A few months earlier Sir John had said, "Taking account of the expected increase in world population, food production must be increased by 110 percent [more than doubled] in the next 25 years if sufficient food is to be provided for all mankind. Only cooperative international action can prevent the direst calamities."[2]

Reiterating in July 1948 Sir John's estimate of what a tremendous 25-year increase in production would be required to provide the world with "sufficient food," and defining sufficiency as a meager 2,600 calories per person per day (meager for the U. S., that is, but not for Oriental populations), President Milton S. Eisenhower, of Kansas State College, added, "And I say in all earnestness that it is an open question whether food production, for all our science, can be increased that much."[3] Reports in September 1948 from the Inter-American Conference on Renewable Natural Resources in Denver, and from meetings of both the American and the British Associations for the Advancement of Science were replete with similar pessimism. A Canadian publication stated in August,

> it would seem, then, that perhaps never again—unless an unforeseen miracle occurs—will the white people of the world be able to enjoy the extremely high level of food that was available to them in 1938 and 1939. The white races . . . will have to adapt themselves . . . to a gradual change in their diet, consuming less livestock products and more cereals.[4]

There is also William Vogt's recent book, *Road to Survival* (New York: Sloane Associates, 1948), described by its publishers as a "revelation of the fact that the earth, as abused by man, is unable to support the human race in terms of its most basic need—food." Fairfield Osborn's book *Our Plundered Planet* (Boston: Little, Brown, 1948, 75–76) chants much the same lament.

> The problem [he says] is how to conserve the remaining good natural soils that exist on the earth, together with the complementary resources of forests, water sources, and the myriads of beneficial forms of animal life. There is no other problem. If that is not solved the threat to human life will grow in intensity and the present conditions of starvation that are already apparent in various parts of the earth will seem as nothing in the years that lie ahead.

Examples need not be multiplied to indicate that we are in the midst of a resurgence of widespread discussion of the old problem of pressure of population upon the food supply. In the English-speaking world three waves of such discussion, heavily weighted with pessimism, have been recognizable since the first one was touched off by Malthus' famous *An Essay on the Principle of Population . . .*, published anonymously in 1798. The second wave came in the late 1890s in connection with the German controversy about the relative merits of agrarian and industrial national economies. Perhaps an ephemeral shortage and high price of wheat was a contributing factor. It was in 1898 that Sir William Crookes delivered his famous address, "The Wheat Problem," to the British Association for the Advancement of Science; some people took it as a "cosmic scare."[5] But again interest in the global food-supply problem waned, only to be stimulated for the third time, for a few years after World War I. We are now in the midst of the fourth wave.

Today

I

What are the sources of this resurgence of interest, and what is new in current discussion?

It appears that many have recently become aware of an increase in births following the cessation of hostilities—at least here in the United

States, in some countries of Western Europe, and in Japan. At the same time it has become clear that war casualties—deaths—were in many countries not so large as to slow down rates of population growth. Also, there has been in the past few years tremendous inventiveness in the field of lifesaving techniques, to mention only the use of the sulfa drugs, penicillin, Atabrine, and insecticides of the DDT type—all tending to reduce mortality rates and all tending to become cheaper and more widely available. Coupling these new developments with widening knowledge of the facts about world population growth during the past half century and more, many come to the conclusion that the population outlook for the next half century is for growth, very rapid and perhaps alarming because the earth's surface is limited in extent.

Certainly the available evidence on world population growth during the past two centuries is impressive. There were perhaps about 875 million people in the world in 1750. Now, as we near 1950, there are perhaps 2,350 million. The words "perhaps" and "about" are used advisedly, for the world's population has never actually been counted. But the estimates, imperfect as they are, seem sound enough to warrant the statements that the world now contains nearly three people to every one person existing two centuries ago, and that the increase, at least if one takes it by centuries since 1650, has been at an accelerating rate in the world as a whole though not in each of its parts.

If we take the present or 1950 world population as about 2,350 million, the probability is that the increase in the past 50 years has amounted to roughly 750 million people, and this in spite of two world wars in the interval. It is an increase of over 45 percent within the adult memories of a good many living men. Much of the world's population growth since 1900 has occurred in areas rather far out on the horizon of our vision, such as India, Burma, Japan, and the Netherlands Indies. Nevertheless, our own national population, helped by immigration, has approximately doubled since 1900, and there seems to be no large geographical area of the world where population increase has not been substantial in the past half century. The rates of natural increase, it is true, were lower in Western Europe, North America, and Australasia than in most other parts of the world. Indeed, not more than a decade ago these low rates of natural increase gave rise to concern about decline of certain national populations in the future. But recently that concern has diminished; the dates of probable maxima of populations have been postponed by the demographers.

One authoritative projection of population growth places the more or less probable world total in the year 2000 as about 3,300 million people.[6] An increase of some 900 million is implied for the half century which we are about to enter—in absolute numbers, a larger increase than occurred in the half century just past. Of course this is not a firm prediction, but it represents the type of projection in the minds of many who currently discuss the problem of population and food supply. They regard population trends as rather intractable and stubborn; and from all we know this seems a proper attitude to assume. Current discussion of the question seems generally to take for granted a great and perhaps unprecedented increase of world population in the next half century, barring only the advent of events highly unpalatable to mankind. Birth rates, it is supposed, cannot reasonably be expected to fall as rapidly as death rates, which are more and more reduced by modern life-preserving techniques—unless indeed the death rates should be elevated by war, famine, or pestilence, all of which men of good will seek to avoid.

Some discussants recognize that rates of natural increase vary from nation to nation, and that the lower rates, presaging little or no augmentation of certain national populations if we neglect immigration, tend to appear in prosperous or advanced countries. Those are countries at once highly urbanized, highly industrialized, highly literate, highly productive economically. Among them are the United States and Canada, Australia and New Zealand, the United Kingdom, Belgium, the Netherlands, Switzerland, the Scandinavian countries, France. Germany was of this group before the war, but may not in all respects be of it now. However, the population of the high-income countries of lowest rates of natural increase of population make up much less than half the world's population; so that an expectation of heavy increase in the world total population, in the absence of unwelcome types of checks, seems well justified.

The war, as everyone knows, tended in many though not all parts of the world to cut down agricultural production, and to curtail the international movement of food from surplus to deficit areas. Post-hostility shortages of many sorts, currency inflations, balance-of-payment difficulties, shifts of national boundaries and population groups, occupation policies, political revolutions, civil wars, and other influences have so far combined to hamper recovery. Additionally, in the world's great

food-importing area of Western Europe, the weather was very bad for crops produced in 1945 and 1947. Only as the harvest of Northern Hemisphere crops of 1948 came to an end did the Western world begin to emerge from a food shortage of great severity. Here in the United States the shortage touched us in imagination and pocket, not in the stomach. But it has certainly struck the imagination hard. From all over the world for nearly a decade stories of the most urgent food needs, or of starvation itself, have poured into our consciousness. We have been made aware, to a degree hitherto unfamiliar, of the generally restricted diets of a billion and a half or more of the world's population even in prewar years. And population growth during the past decade of war and incomplete recovery has tended to accentuate the meagerness of food supplies in all but a few exceptionally situated food-exporting nations like our own. No wonder, on this count alone, that pessimism rules.

With growing population coupled with lagging recovery of food output and distribution, it appears difficult for the less fortunately situated nations to recoup the level of food intake prevailing before the war, in the late 1930s. But, in the minds of many, perhaps especially the nutrition-minded, such recovery is not enough, not what the world wants or needs. Rather, the desideratum is substantial improvement in the per capita food intake of most nations, perhaps of all—one aspect of "Freedom from Want." This is an idea for several years strongly espoused by the Food and Agriculture Organization of the United Nations. It has spent no little time and effort in publicizing the facts that even before the war, as measured against standards of adequacy set up by the nutritionists, the average per capita food intake of many nations, comprising in total far more than half the world's population, was sadly deficient in quality and not infrequently in quantity; and that even in the best-fed countries at least a modest fraction of the population was either undernourished or malnourished. Although evidence for these conclusions began to appear nearly two decades ago, it has lately been the FAO more than any other organized group that has broadcast information on the quantity and composition of national food supplies. And it has espoused as its foremost objective, its major *raison d'être,* the improvement of human nutrition throughout the world—not merely the recovery of prewar nutritional status.

If those who limit their hopes to a fairly prompt return of the world to its prewar nutritional status are nowadays pessimistic in outlook, we need not be surprised that those whose hearts are set on general improvement of nutritional status are even more so.

The jeremiads of the nutritional idealists, however, are hardly as frightening as those of the conservationists, notably the soil conservationists. Extremists of this group seem to take for granted the population projections of the demographers and the nutritional goals of FAO; and then they proceed to uncover, it would seem practically everywhere, evidence of permanent soil erosion by water and wind, and of depletion of soil fertility. Both are ascribed in dominant part to blind pursuit of profits by the farmers and graziers of the world, which is alleged to lead to mistreatment of the soil.

There seems to be a degree of historical coincidence in the rise of the soil-conservation school to its present degree of public prominence. Great dust storms afflicted our Great Plains in the middle 1930s. A bit earlier, in 1933, this nation had embarked upon the adventure of curing economic depression in part by supporting farm prices and incomes, and the method first chosen was to make Federal payments directly to farmers in exchange for acreage reduction. When the Supreme Court in January 1936 put a stop to this "gentle rain of checks," as a shrewd observer called it, a lawful way to continue became the object of political expediency. And so the payments earlier made to purchase acreage reduction became "agricultural conservation payments;" politics opened the door of the public treasury to conservationists; the great droughts carried conviction to the public of a crying need for conservation; and increasingly since then political leaders, soil conservationists, and the public seem to have been at one in viewing soil erosion as a dreadful threat domestically. As conservationists proliferated, the perception of threat to food supply through soil erosion spread so as to compass whole continents. The books by Vogt and Osborn provide outstanding, perhaps extreme, examples of literature originating with the ardent-conservation school. That group, of course, cries for "action now," and for funds as well. Allusion is made to civilizations which, it is claimed, disappeared because their soil was swept away. It is sought to check or control erosion, thus saving civilization.

The soil-conservation school adds to the old concept that the food-producing land of the world is strictly limited in extent the new concept that the land is actually being destroyed, and at a rapid rate. The argument is picked up in strange places. An

advertisement of a British firm selling agricultural machinery reads in part: "To avert mass starvation and world strife, more food must be produced. Man in his ignorance, however, has raped vast areas of the good earth. Soil erosion, like a cancerous growth, has doomed miles of the land to sterility."[7]

The broad and general tenor of current discussion is summed up in a sentence of Sir William Crookes', spoken in 1898: "As mouths multiply, food resources dwindle."[8] A few points made in this fourth wave of pessimism, however, seem new. Concern has largely shifted, for example from "bread-eaters" of the Western world to the teeming populations of Asiatic countries. Again, interest has largely shifted from mere maintenance of accustomed diets to the improvement of diets. "Improved" diets usually involve enlargement of the proportion of calories derived from animal products; and this would involve extra drafts on productive resources because the animals burn up humanly edible grain when they convert it to meat, milk, or eggs. Sir John Orr may be thinking of such dietary improvement rather than maintenance, or even return to pre-World War II status, when he estimates that world food production must be increased by 110 percent in the next 25 years if sufficient food is to be provided for all mankind. New also in current discussion is the heavy emphasis on destruction of soil through erosion. And, finally, one sees little in earlier discussions the idea, often mentioned now, that there would be little purpose in fostering the international spread of new lifesaving techniques and sanitary measures, because resulting increased pressure of population on food supply would merely plunge into misery those whose lives were saved. The emergence of some of these new notes tends to intensify current pessimism. A vicious circle as perceived nowdays seems, if not altogether different from what it seemed before, to be more vicious than we used to think.

The consequences envisaged in current writings are various, some perhaps rather vague. Sir John Orr speaks of "direst calamities" in the absence of concerted international action, but is not specific about the nature of those calamities or about the international action. Eisenhower speaks of "the specter of permanent, world-wide hunger"[9] as now seeming very real. Our British advertiser speaks of the necessity of greater food production if "mass starvation" and "world strife" are to be averted. Vogt, Osborn, and Eisenhower alike make much of the disappearance of earlier civilizations, as in Mesopotamia, Syria, Rome, northwestern China, and Guatemala, and they suggest that modern civilization may be headed in the same direction. Deserts are said to be on the march, because men disturb and destroy natural vegetation. Less frightening though perhaps still unpalatable is the Searle Grain Company's opinion that the people of the world may never again enjoy as good a food supply as existed just before World War II, and that the white races in particular may have to cut down on ingestion of livestock products and resort more to grain.

II

It seems pertinent next to review sketchily what has happened to food supplies or intake over the world in the past 50 years or so when population increased by perhaps 750 million people. The recent past has some bearing on what may conceivably happen in the next 50 years when, the demographers suggest, a further increase of some 900 million may reasonably be expected in the absence of intolerable "positive" checks to population growth.

The question posed with respect to the past half century has to be stated with more precision. Not much is to be learned by asking what has happened to the average per capita calorie intake of the whole world, or what has happened to the composition of the diet of the world as a whole. We simply do not know, and cannot expect to find out. But there is no point in raising such a question. The world is nothing like a unit with reference to food intake,[10] any more than it is with reference to per capita real income, and a per capita world average is next door to meaningless. Much more useful to raise are the questions: Where throughout the world has food intake become more plentiful and more varied in composition in the past half century? Where less so? Where unchanged? And why?

Simply because the only available information pertains to nations or colonies, the changes must be located mainly in terms of national boundaries —and even this only approximately because national boundaries themselves have changed.

A degree of precision will be added if we mainly consider two aspects of average national diets: (*a*) the average per capita intake of total food calories from whatever crude foodstuff they may be derived; and (*b*) the proportion of national food intake derived from the grains and potatoes (including here such items as cassava and taro). It may be assumed that a long-persisting decline in

the "cereal-potato" fraction of a national diet represents an improvement of diet unless there is evidence of accompanying exceptional decline in total calorie intake per capita. For the cereals and potatoes are almost universally the cheapest of all foods per thousand calories; and all population groups except the very wealthy clearly tend to reduce the proportion of total calories derived from cereals and potatoes whenever they can do so, expanding variously the proportions of their food-calorie intake derived from other principal and more expensive groups of foods: namely, sugars, vegetable oils, legumes or pulses, low-calorie fruits and vegetables, and animal products, including meat, dairy products, eggs and poultry, fish, and their accompanying fats. A decline in the proportion of calories derived from cereals and potatoes in a national diet does not constitute altogether conclusive evidence of nutritional improvement of diet; it may not, especially if only sugar or vegetable oil replaces the cereals and potatoes. But in general a decline of the cereal-potato fraction of a diet means increase of palatability and diversity and represents economic improvement of diet. On all the evidence we have, the mass behavior of people is to reduce the cereal-potato component of diets when that component is high, if they can afford it. Such evidence comes forward in all surveys of food consumption by differing income groups, in whatever places and at whatever dates the surveys are made. But the tendency does not run to complete displacement; perhaps 10–20 percent of cereal-potato foods would be retained by a mass of millionaires.

At the present time it happens to be impossible to ascertain, systematically, nation by nation for the whole world, exactly what changes have occurred in per capita calorie intake or in the proportion of calories derived respectively from cereals and potatoes and from other foods. If estimates of population growth over the past half century are commonly lacking or uncertain, the position is far worse with respect to food supply. We know most about the nations where the record keeping is best; where domestic crops have long been painstakingly estimated, and imports and exports systematically recorded. The measurement of a national food supply is fearfully complex to accomplish, not only because there are so many components in a food supply, but also because so many important foodstuffs are usable and are used both for food and for feed.

More is perhaps known about the United States than any other country. Elaborate official statistics and estimates cover, year by year, the period since 1909. They show, in the 30 years between 1909 and 1939, a small decline in total calories per capita "available . . . at the retail level" from about 3,500 to about 3,300. They show also a much more striking decline in the proportion of total calories derived from grains and potatoes—from 44 to 29 percent. On the other hand, there were measurable increases in the proportions derived from sugar (from 12 to 16 percent), from fruits and the vegetables other than potatoes (from 7 to 10 percent), and from meat, fish, poultry, eggs, dairy products, and fats and oils (from 37 to 45 percent). Thus the proportion of national calories derived from grains and potatoes fell, and the proportion derived from other foodstuffs, more expensive per thousand calories as a group and with respect to nearly all individual items, reciprocally rose—at least from 1909 to 1939.[11]

If one now seeks to push backward in time through the 20 years preceding 1909, the official statisticians give less help. But Holbrook Working[12] was able to establish convincing evidence of decline in the per capita consumption of the chief grain products, wheat flour and corn meal, and also an increase in per capita consumption of sugar, during those two decades. Flour and meal consumption fell from about 560,000 calories per person per year in 1889 to about 450,000 in 1909, a decline of nearly 20 percent. The rise in sugar consumption did not offset this. Working's incomplete findings are consistent with the interpretation that general trends known to to exist from 1909 to 1939 existed also from 1889 to 1909—a decline in total food calories per capita, a decline in the proportion derived from grains and potatoes, and an increase in the proportion derived from other foodstuffs than grains and potatoes, demonstrably from sugar.

These, then, were the tendencies in the United States for half a century preceding World War II. They represented a general sufficiency of total calories even with decline; that decline is reasonably attributable mainly to lowered requirement due to reduction of physical activity—machines increasingly working for men. They also represented wider freedom of choice in foods, greater variety and palatability—in short, an improvement of diet in the economic sense, whether or not in the nutritional sense. This could not have happened without increase in per capita real income, of which there is plenty of evidence that need not be brought forward.

But the United States is only a part of the

world. What happened elsewhere? Of this we know less. It is a proper subject for prolonged and intricate research. Yet a good many pieces of evidence can be tied together to indicate that decline in the cereal-potato fraction of national diets was rather widespread in the half century before World War II. Quite commonly we perceive in statistics declines in per capita human consumption of what are generally regarded as inferior cereals—rye, corn, barley, oats, and the millets and sorghums. In itself this indicates the existence of economic circumstances permitting wider choice of more expensive and desirable foodstuffs, among which the preferred cereals, wheat and rice, commonly appear. Quite commonly we perceive increases in per capita consumption of sugar. Not uncommonly we perceive declines in per capita consumption of a preferred cereal, wheat; but this reflects a reciprocal rise in consumption of more expensive foods. Trends toward economic improvement of national diets can be demonstrated satisfactorily at least for Canada, Australasia, the British Isles, Scandinavia, Germany, the Low Countries, Switzerland, France, *and Japan*—in kind resembling the trends in the United States, in degree and in details quite likely different. One need not much hesitate to add to this list such nations as Argentina, Brazil, Mexico, Cuba, the former Baltic States, Poland, Czechoslovakia, Austria, Hungary, Yugoslavia, Greece, Italy, Spain, and Turkey. In each of these, there appears to be no suggestion of a long-term trend toward insufficiency of total calories per capita, even if there was slight decline as in the United States and for similar reasons; and there is some evidence of increasing diversity of diet.

Again looking at the half century preceding World War II, we see, though most dimly, developments which have the appearance of being somewhat different in several Oriental countries—India, Burma, Korea, Formosa, Java, the Philippines. The meager statistics for recent decades—one cannot press far back—suggest a gradual small reduction of per capita calorie supply, coupled with tendencies for inferior cereals and for sweet potatoes to increase while the favored grain, rice, declines in consumption.[13] In spite of some small increase of wheat consumption, occasionally of sugar, and in spite of lessened incidence of local famines, one finds little direct statistical evidence of increasing per capita consumption of fats, meat, dairy products, and fish. The dependence on cereal-potato foodstuffs remains very high; if it declined, the decline was small. There is a suggestion here, though only a suggestion, of general tightening of the food situation—fewer calories where more were needed—rather than enlarged scope to diversify diet.

The largest population groups not yet mentioned are China, the African continent, and the USSR. On food developments in China and Africa, little useful statistical evidence is to be found. That northern China suffered sporadic localized famines in the course of the half century is certain. Whether there was improvement or deterioration generally in calorie supply or in composition of the national average diet is unknown. There is perhaps evidence of improvement in African food supplies, but no conclusions need be drawn here.

In the USSR, from 1898 to World War I, there seems to have been general sufficiency of calories aside from local shortages, also a shift from rye to wheat, and possibly a degree of increased diversification of dietary composition. The first world war and the Revolution led to famine in 1921–23. Subsequent recovery was interrupted by the drive for collectivization and ensuing slaughter of livestock, and another famine occurred in 1932–33. Again came recovery, but Soviet statistics are not such as to tell us truly about calorie supplies and composition of diet just before World War II.[14] There is ample reason, however, to believe that the rural fraction of the population was then less well fed than it had been before World War I; and the rural fraction remained, in spite of growing industrialization, much the larger fraction of the total. Also, the urban population itself seems unlikely to have been as well fed before World War II as it was before World War I.

So much for the history of the relationship between population and food supply during the half century preceding World War II. The world population growth was enormous. But meanwhile food intake improved in a good many national populations. This, it should be emphasized, is the fact so far as concerns the regions about which we know most and can have most confidence in the statistical evidence. As for the populations where deterioration of the food position is suggested, either the credible direct evidence on food supplies is decidedly meager, or we can be fairly certain that the deterioration sprang in large part from governmental interventions, as in Korea, Formosa, and the USSR.

Even with respect to India, one cannot feel confident that the food position really deteriorated progressively, although the statistics—such as they are—point in this direction. For we know that at the same time India unquestionably became

increasingly urbanized and industrialized. Except as this comes about through a socialized control—investment squeezed from consumption—such as prevailed after 1917 in the USSR but never in India, urbanization and industrialization spell enlargement of national real income per capita; and that tends strongly to carry with it improvement of diet in the economic sense. It is difficult to believe, of India or any other nonsocialized country, that city populations grew as they did in relation to rural populations for any other important reason than that the farm people saw a chance to improve their income status in the cities, and that the process of industrialization opened avenues for them. The people who migrated from farm to city were not jumping from frying pan to fire. On this line of reasoning, one cannot feel fully confident that India, and perhaps, though less probably, even China, were not enjoying a somewhat more diversified average national diet, albeit a poor one by our standards, in 1939 than 50 years before. The facts are not clear, whereas the facts *are* clear concerning the great majority of nations belonging to the commercial Western world.

The half century preceding World War II may then reasonably be regarded as one more accurately characterized as giving evidence of improvement in per capita food supplies than the opposite. Widespread indeed were demonstrable improvements, whereas positive deterioration suggested in some places is subject to doubts not readily dispelled, or can be explained in terms of political interference with economic development. That this is the picture that emerges is the more remarkable because a destructive world war occurred in the interval, to say nothing of minor yet devastating localized wars; and because a rather general economic paralysis beset the world toward the end of the 50-year period. The forces that make for higher levels of living, with accompanying dietary improvement, must have been tremendously powerful.

III

It would be possible to review in a general way what happened in the 50 years preceding 1939 to improve national food situations in so many parts of the world. One would mention, as did Joseph S. Davis in his explanation of history's answer to Sir William Crookes,[15] not only expansion of acreage but increase of yield per acre, through improved breeds and varieties, better rotations, the spread and cheapening of transportation, the invention of laborsaving machinery, and so on. It may be added as factual material that between 1888 and 1938, even in long-settled areas of the world excluding China and Russia, it proved possible for the yield per acre of wheat to increase fully 25 percent while acreage increased some 10 percent; and in the new areas of the world, although expansion of acreage supposedly to poorer and drier land was no less than sixfold—more than 100 million acres—the yield per acre nevertheless did not decline if one makes allowance for the unusual character of the droughts of the middle 1930s in the Great Plains.[16]

But this approach is perhaps no more illuminating than to ask the question, What is it that was impeding before 1939, is now impeding, and may well continue to impede the growth of economic productivity in general and of agricultural production specifically? What, in particular, tends to prevent more food and more varied food from being produced and put into the hands of the multitudes who need or desire it?

Those impediments are overwhelmingly numerous if looked at one by one. Economists long since invented, however, a set of rubrics convenient for the discussion but including only four terms. They would say that agricultural productivity, with reference to an unchanging supply of land, must depend upon the application of the three other factors of production—labor, capital, and management. The rubrics—land, labor, capital, and management—are convenient, if only because bearing them all in mind precludes our focusing attention upon only one or the other of them, as I suspect some soil conservationists do.

About any acre of agricultural land now in use in the world, theory and history tell us (1) that it has a *maximum* theoretical (economic) productivity, given the state of technology of the moment; (2) that its maximum theoretical productivity may or may not actually be achieved at the moment, depending upon whether the optimum combination of labor, capital, and management is being applied; and (3) that in all probability, as judged by history, the point of maximum productivity will be shifted upward as time passes because technological advances will be made, even if we cannot predict either the degree or the pace of advance. But nothing in history or theory tells us what the output may be, in volume or value, at the point of maximum productivity.

The most impressive probability about the acres of the world now devoted to agricultural use is that a truly enormous gap exists between

actual productivity and maximum productivity under optimum application of labor, capital, and management.[17] This refers to maximum economic productivity, not maximum physical productivity, which no doubt is the greater. In all probability a very large fraction of the managers of the world's agricultural land are inhibited from applying optimum doses of labor, capital, and management. Either they are not aware of the best in current technology, or they are helpless to make use of it, or they are unwilling even if able. The mere existence of cover crops and green manures, commercial fertilizers, effective sprays against weeds whether new or old, hybrid corn, rust-resistant wheats, inoculation against hog cholera, tests for animal tuberculosis, is literally unknown to thousands of land managers in the very areas of the world where population is alleged to be most obviously outrunning food supply. In those same areas and others are land managers who know of an improvement, but have no incentive to put it to use. Somewhere a large landholder, wealthy enough and socially important enough to satisfy all his ambitions, may remain content with farming methods reminiscent of the Middle Ages. Perhaps one could find instances in Spain or Chile. Elsewhere a lack of security of life and property—in essence, a lack of orderly government—may inhibit the application of improvements widely known. Bad though orderly government, exercising unwisely its power of taxation, may inhibit improvements in farming, simply because the fruit of effort is not permitted to accrue to the men who make it. The Soviet Union is a case in point. Above all, perhaps, a great many land managers of the world cannot apply what they both know and are willing to try for the reason that they lack capital to cover the initial cost. The purchase of a pound of improved seed, to say nothing of a good spade or plow, may unfortunately be quite beyond the capacity of a great many who cultivate the world's soil.

For these and other reasons, the gap between agricultural potential and agricultural practice is unquestionably very wide, and has been very wide at any given point of time for far more than half a century. One of the other reasons, a most compelling one, is lack of demand or of the development of demand. There has to be an exchange of products between those who manage the land and those with whom they exchange produce; and the last must have something to exchange. Agricultural productivity not only responds to advances in economic productivity on other fronts, but stimulates it as well. The process is complex in the extreme, involving of necessity appropriate fluctuations in the relative prices of the factors of production; but its existence ought not to be overlooked merely because it is difficult to describe and trace. The invention and use of a pound of higher-yielding seed, of a longer-lived and sharper hoe, or of a way to twist rope at lesser cost, work reciprocally to enhance both agricultural and industrial productivity. Multiplied in thousands of instances, such innovations exemplify economic development. More elaborate division of labor, a most excellent way of increasing economic productivity, is an accompanying phenomenon. The race is less accurately between population and food supply, than between population and economic development in every sphere. And even here the analogy is imperfect, for the racers are hobbled together: economic development at some stage becomes associated with a slowing down of the rate of population growth.

Much that can be said about the gap between agricultural practice and agricultural potential may equally be said about a gap between food harvested and food eaten. Losses in storage and transportation are truly impressive, and are unquestionably subject to reduction as the effectiveness of inhibiting influences is removed or lessened.

So far the emphasis has been given to factors that impede the increase of food yield per acre on land under cultivation now. It remains briefly to speak about land itself, the lack and the destruction of which are currently so heavily emphasized as impediments to improvement of per capita food supplies. Almost axiomatic is the fact that there is now less naturally good land for men to bring into use than ever there was before in historical time. Far from true would be a statement that there remains no unused land whatever subject to development for food crops. There are still areas of the world where lack of transportation hampers settlement, and where the presence of the tsetse fly or the malarial mosquito acts similarly. No one could reasonably suppose that the tropics are now fully developed, with reference especially to oil-bearing palms. Also the limit of land cultivation—at best a vague concept—is elastic, not fixed. It changes, for example, whenever a shorter-maturing variety of grain is invented, moving the limit both in latitude and in altitude—and even, perhaps, permitting double cropping of land once single-cropped. If the behavior of demand is such as to call all these reservoirs of food production into activity, their

product will be huge indeed. Economic planners in Brazil estimate that Brazil has 150 million acres fit for growing wheat. Those acres may never or may not soon be settled, but the estimate itself is indicative of ideas held by some people that by no means all the productive land of the world is as yet under the plow. And land is not all. One cannot dismiss as inconceivable the profitable use of the plankton of the oceans as food or feed; and ways and means of utilizing solar radiation to provide the energy for artificial synthesis of the food elements remain to be speculated about.[18]

Some writers, such as Osborn and Vogt, who have espoused the cause of soil conservation, paint a picture of rapid contemporary destruction of soil resources through erosion by water and wind. The question here is not whether there is any erosion at all, for there is, but how important it may be; and this is a difficult question. If a man alleges, as Vogt does, that in this country we have lost a third of our topsoil in 150 years and that the future of our country is still within our control but will not be after only a few more decades of such abuse as we have subjected it to, no easy counterassertion comes to mind. It is comforting then to read the words of an experienced soil scientist, Charles E. Kellogg, chief of the Division of Soil survey in this country (*Sci. Mon.*, 1948, **66**, 478, 479): ". . . a large part of the arable soils of the world are made better by good farming than they were naturally." And to have him say, "For a time, it almost seemed that each popular writer was trying to outdo the others in dramatic statement. Some even went so far as to assert that erosion had swallowed whole civilizations. . . . It is sincerely to be hoped that the period of extreme statement on soil erosion is nearly over. . . ."

One can hardly fail to be impressed not only by the tendency of the extremists to engage in what may be called spurious quantitative thinking, but also by their failure to make clear precisely where all the damage they see has been done, or to differentiate between geological and man-made erosion, or to stress what has happened on the great agricultural flatlands of the earth equally with what they say has occurred on the much less important sloping uplands, which are singled out as manifesting the soil-destroying evils of so-called "fire farming" or "shifting cultivation." On reading the extremists, one would never guess that the Missouri River was carrying silt—was called the "Big Muddy"—when men who knew the plow or herded domestic animals first saw it; or that beavers had ever made dams which silted up and formed mountain meadows long before the mountains above were grazed; or that dust storms had occurred in the Great Plains in the 1890s, before they felt the plow.

One who has no claim to expertness in this field may neverthless find it impossible either to deny the existence of man-made soil erosion and the desirability of a degree of social action against it, or to become greatly exercised about it when he thinks of the impressive array of quite different factors which inhibit agricultural productivity.

IV

What need be said in conclusion? Certainly it seems futile to engage in unqualified prediction in so inexact a field of inquiry, and it would be tiresome to state all the qualifications necessary in prediction. We must concede, perhaps, that the "direst calamities" lie in the realm of possibilities, though in appraising even possibilities it would be helpful to know more precisely what those calamities are thought to be. If one of the "direst calamities" is eventual reduction of the per capita ingestion of animal products among the white races, we must concede its possibility; and so with "mass starvation" if by that is meant what has long occurred locally and among relatively few people at any point in time. To concede possibility in these matters, however, is not to argue probability.

We need not concede the possibility within the foreseeable future of the emergence of "permanent world-wide hunger," if that phrase means everybody hungry at once. That cannot happen unless a world society develops and finds a way to achieve absolute equality of personal income, a possibility surely so remote as hardly to be called a possibility at all. In such a society the tremendous contents of the feedbin of the world could be utilized to assuage human hunger; humanly edible grain now lost to man in the conversion processes of animals could then be captured for human consumption.

We need not concede that possible localized national deterioration of diets of historical type leads at all certainly to world strife. Hungry nations are commonly economically weak and weak in military potential as nowadays measured in other terms than clubs or knives, and hence may be chary of assuming the risk of aggression; and in any event wars have other causes than scarcity of food.

We might not be wise to concede, on the other hand, the possibility of an increase of 110 percent in world food supplies within 25 years. That is a decidedly short time for so tremendous an increase.

And, yet, at the same time that we concede the possibilities both of deterioration of food supply and of the unlikelihood of hoped-for extreme improvement, we need not exclude the possibility of a geographically spreading gradual improvement of national diets, doubtless unequal in pace among the several nations, resulting in generally firmer assurance of food supplies increasingly consistent with human aspirations. It is well to remember that the recent years of food shortage may, if a long stretch of peace lies before us, seem in retrospect merely transitory.

Pessimism about maintenance or improvement of per capita food supply, even where population is densest, is not intellectually necessary, not compelled on the basis of historical fact or logic. We know approximately what the trends have been. More important, enough is understood about the powerful forces lying behind economic productivity and dietary improvement to warrant the statement that the ultimate productivity of the earth's surface had best be regarded as incommensurable and elastic, not fixed. It can be measured, and thereafter set in relation to a future growth of population itself not firmly predictable, only in terms of surface—square miles or square feet. The customary but spurious quantitative exercise—only 4 billion cultivable acres now, less than 2 acres per person, with 2.5 acres estimated to be required per person to provide a minimum adequate diet, and with population growing while soil is washed away—ought not to mislead. The possible product of the earth defies measurement even on the assumption of stagnant technology, and the assumption itself gains no support from history.

Time may prove today's pessimists to have been right in their attitude; the actual outcome remains to be seen. But if they prove right, the historical reasons are quite unlikely to be limited to an over-rapid increase of population set against an over-rapid destruction of soil resources. The future historian will need to inquire whether some other contributory causes were important—whether men and governments acted wisely in such directions as engaging in wars, keeping civil order, spending appropriately on education and research, taxing with propriety, intervening intelligently in economic life, nursing economic isolation, or freeing the channels of trade. To follow the wrong paths is to hamper invention, stifle capital accumulation, hinder investment domestically and internationally, and hence to retard the general economic development, one aspect of which is improvement of national diets. If the right paths are followed in these directions, and not only in human reproduction and in land use, time may prove today's pessimists to have been wrong, as with the pessimists of yesterday.

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SOME ADVANCES IN CHEMOTHERAPY

MONROE D. EATON

Dr. Eaton (M.D., Harvard, 1930) has been associate professor of bacteriology at Harvard Medical School since 1947. For the preceding ten years he was director of the Virus Laboratory of the California State Department of Health and a staff member of the International Health Division of the Rockefeller Foundation.

THE dramatic advances in the treatment of bacterial infections with the sulfa drugs, penicillin, and streptomycin do not, unfortunately, extend to the filtrable viruses. The viruses causing diseases such as the common cold, influenza, poliomyelitis, encephalitis, and yellow fever are not affected by any of the new drugs or antibiotics. There are, however, certain minute intracellular parasites that resemble both bacteria and filtrable viruses, and infections caused by them are accessible to chemotherapy. In order to define the problem more clearly let us first briefly summarize our present knowledge of the nature of viruses.

The filtrable viruses differ from bacteria in several properties. The size of the smallest bacteria is in the range 125–250 millimicrons. The largest viruses and the related rickettsiae are about the same size or slightly larger than the smallest bacteria and, like them, can be seen under the ordinary microscope. Other viruses range in particle diameter from 150 to 10 millimicrons, or down to the size of the largest protein molecules.

From our present knowledge of filtrable viruses, it is apparent that this group may contain infectious agents of very diverse nature. In fact, the differences among the various filtrable viruses are probably of much greater magnitude than the differences among species of bacteria. Some of the largest viruses are coccoid, or spherical, in shape, and the rickettsiae are rod-shaped; these infectious agents resemble bacteria both in their morphology and staining characteristics. Certain other viruses as photographed under the electron microscope have quite a different morphology from bacteria. The viruses of smallpox and cowpox are also among the largest, but they have less resemblance to bacteria, being cuboid, with several internal round dense areas. Some of the bacteriophages (viruses which infect bacteria) are ovoid in shape, with a flagellum, or tail.

Crystallization of two of the plant viruses, tobacco mosaic and bushy stunt of tomatoes, and their characterization as nucleoproteins have led to interesting hypotheses about the nature of these agents as nonliving, self-propagating substances. It is not within the scope of this article to discuss these studies with plant viruses except to relate their implications to similar studies on animal viruses. The virus of tobacco mosaic separated from crude infective sap by ultracentrifugation and chemical procedures is obtained in the form of long fibers pointed at each end. These are now generally spoken of as paracrystals. The virus particle itself is rod-shaped, with a diameter given as 15–20 millimicrons and a variable length of 37–1,400 millimicrons, depending upon the state of aggregation. The virus of bushy stunt of tomatoes has been obtained as true cuboid crystals; the particle of this virus appears to be spherical, with a diameter given as 30–40 millimicrons. Both these plant viruses have been prepared in a state of purity such that no protein constituents derived from the plant tissues can be detected by delicate immunological methods. Chemical analyses reveal no component in these viruses other than protein and nucleic acid. These and other properties of the plant viruses seem to set them apart from animal viruses and bacteria.

The animal viruses have not been obtained in a comparable state of purity. Several have been purified to the extent that they may be identified as characteristic forms under the electron microscope, give single boundaries by ultracentrifugation and electrophoresis, and show a fair degree of uniformity in chemical composition. Estimates of the amount of impurities derived from the host tissues vary from 20 percent to several hundred percent of the content of virus particles. Chemical analyses must therefore be considered less significant than in the case of the plant viruses, but major quantitative differences in constitution between the virus and infected tissue have been found. The larger viruses such as influenza and vaccinia (cowpox) contain nucleoproteins, lipids, cholesterol, neutral fat, and carbohydrates, and their chemical constitution appears to be not very much different from that of bacteria. Bacteriophages thus far

studied appear to contain much larger amounts of nucleic acids than other viruses or bacteria. Copper, biotin (a vitamin of the B group), and a coenzyme (flavine-adenine nucleotide) have been demonstrated in vaccinia virus. Purified viruses, unlike bacteria, exhibit no enzyme activity or metabolism, yet it seems likely that at least some of these agents are metabolically active while growing inside the host cell.

At the borderline between virus and bacterium are the rickettsiae, which cause typhus, spotted fever, and other diseases in man, and a group of coccoid agents, the so-called psittacosis-lymphogranuloma group (Chlamydozoaceae), which cause pneumonia, venereal disease, and eye disease in man and also produce infections in birds and animals. These agents are apparently obligate intracellular parasites like other filtrable viruses and are therefore not as accessible as bacteria to serum antibodies and chemical inhibitors. In other respects they resemble bacteria. In sections of infected tissue fixed and stained by appropriate methods, rickettsiae are found, in masses resembling colonies, in the cytoplasm or nucleus of cells. Similar microcolonies of the elementary bodies of the chlamydozoaceae may be found in the cytoplasm, and recent evidence indicates a life cycle for these viruslike agents. The colonies apparently develop from a minute initial body by repeated fission until the infected cell dies and bursts. The liberated elementary bodies then enter adjacent cells, form new initial bodies, and the process is repeated. Certain bacteria, such as the bacillus causing tularemia, will, under some conditions, multiply in the cytoplasm of cells; there are other relatively large infectious organisms, such as the leprosy bacillus and the plasmodia of malaria, that are, like the rickettsiae and viruses, obligate intracellular parasites. Most bacteria by contrast multiply in the intercellular fluids of an infected animal and are seldom if ever seen in the cytoplasm of cells, except when taken up by phagocytes. With few exceptions, bacteria can be cultivated in lifeless media of varying degrees of chemical complexity.

Some of the rickettsiae and the chlamydozoaceae are susceptible to the action of the same chemotherapeutic agents that have been found effective for the treatment of bacterial infections. Soon after their discovery sulfonamides were used with moderate success in the treatment of lymphogranuloma venereum and trachoma, diseases caused by members of the chlamydozoaceae. This observation emphasized the close relation of these larger viruses to bacteria. Further investigation revealed that certain members of this group were almost as easily inhibited as bacteria by sulfonamides. The rickettsiae are not affected by the sulfonamides, but a related substance, sulfonamidobenzamidine, has been reported to be active against experimental typhus infections in mice.

Penicillin is inhibitory in varying degree to most of the chlamydozoaceae and to some of the rickettsiae in experimental infections of mice and chick embryos. This antibiotic has been used with apparent benefit in the treatment of virus pneumonia caused by psittacosis and related agents, but the required dose is much larger than for bacterial infections. Although rickettsial infections in chick embryos are retarded by either penicillin or streptomycin, these antibiotics have not found successful application in the treatment of rickettsial diseases in man. Streptomycin is apparently inactive against the chlamydozoaceae. In general, therefore, the sulfonamides and antibiotics have been found more limited in effectiveness against these borderline agents than against bacteria. A recently discovered antibiotic, chloromycetin, shows marked chemotherapeutic activity against rickettsiae, and field studies on human disease are at present being done by members of the research staff of the Army Medical School. Another antibiotic, aureomycin, discovered within the last few months, is reported in preliminary experimental studies to be effective both against the rickettsiae and the chlamydozoaceae.

In addition to the antibiotics and sulfonamides, other compounds that inhibit the rickettsiae and the chlamydozoaceae have been found. Several of these substances are more active against experimental infections with these intermediate agents than against bacterial infections. It has recently been shown that certain of the acridines, a class of substances used for some time in protozoal diseases such as malaria and trypanosomiasis, are moderately effective against experimental infections with the rickettsiae and the chlamydozoaceae. The activity of various acridines against these agents apparently does not correspond to their activity against malaria parasites. Atabrine, an effective antimalarial, has no significant activity against the rickettsiae and chlamydozoaceae, whereas a drug called nitroacridine 3582 inhibits these agents but is relatively ineffective against experimental malaria. Another class of substances which has been found to possess therapeutic activity in experimental infections with the rickettsiae, but is inactive against the chlamydozoaceae, is the thionine dyes, methylene blue, toluidine blue, and certain

related compounds. In their chemical structure these dyes resemble the acridines, and also in a general way the B vitamin riboflavin.

Para-aminobenzoic acid prevents the bactericidal effect of sulfanilamide and related compounds, and is a component of the vitamin folic acid. Presumably, sulfanilamide inhibits the growth of bacteria by interfering with their metabolism of *p*-aminobenzoic acid, as will be discussed in more detail. In the rickettsial diseases *p*-aminobenzoic acid is an active chemotherapeutic agent, but sulfanilamide is without effect. Para-aminobenzoic acid has been used clinically with some benefit in the treatment of typhus fever and scrub typhus. The therapeutic effect in man, however, is not as striking as in experimental animals.

Certain hypotheses about the mode of action of some of the rickettsiostatic drugs merit discussion because they introduce a new concept applicable only to obligate intracellular parasites: namely, that substances like *p*-aminobenzoic acid and toluidine blue modify the metabolic processes of the infected cell rather than act directly on the infectious agent. Recent observations have shown that *p*-aminobenzoic acid stimulates oxygen consumption by embryonated eggs, whereas rickettsial infection depresses the consumption of oxygen. Toluidine blue is also known to serve as a readily reversible carrier of hydrogen in oxidation-reduction systems. It is possible, therefore, that these substances, by increasing oxidation processes of infected cells, produce unfavorable conditions for the growth of rickettsiae, or furnish the infected cells a metabolic pathway alternative to those enzyme systems destroyed or impaired by the infection. Increase in temperature or supply of oxygen also tends to raise the metabolic rate and increase the survival rate of chick embryos and mice infected with rickettsiae. Substances such as potassium cyanide, which in sublethal amounts depress the oxygen consumption of embryonated eggs, seem to favor the growth of the rickettsiae. It is conceivable that chemotherapeutic agents could also beneficially modify or supplement various cellular enzymatic processes other than those concerned with oxidation.

Most of the substances mentioned, and many others, have been tried in experimental infections caused by the smaller viruses, but the results have been negative or inconclusive. They are also ineffective against the virus of vaccinia and related agents, which are apparently as large and as complex as the chlamydozoaceae. Possible exceptions are certain of the acridines and a drug named hexamidine. The latter substance and the previously mentioned nitroacridine 3582 are reported to have some inhibitory effect on the virus of influenza in embryonated eggs, but these observations lack confirmation. Other acridines inhibit the growth of bacteriophage without corresponding diminution of growth of the bacterial host cell.

Some success has been attained by following a concept involving a somewhat different approach to the problem, namely, attempts to block the attachment of the virus to the host cell. The viruses of influenza and mumps are adsorbed on the red blood corpuscles of certain species and agglutinate these red cells. It has been found that the attachment of these viruses to red cells is prevented by certain polysaccharides, or complex sugars, and by extracts of red blood cells and bacteria. Some of these substances inhibit the multiplication of the viruses of influenza and mumps in embryonated eggs.

Since the influenza virus is considerably smaller than the intermediate agents discussed above (although it superficially resembles a minute bacterium in morphology and chemical composition), successful inhibition of this virus would represent a step beyond the borderline. Scientists at the Rockefeller Institute for Medical Research have found that pectin, a complex carbohydrate from fruits, and related substances prevent infection of influenza virus in embryonated eggs when given in large doses half an hour before, or up to two hours after, the virus. Workers in Australia have obtained similar results with what appears to be an unrelated agent. Extracts of cholera vibrios and certain other bacteria were found to contain an "enzyme" that destroyed the receptors by which the virus became attached to red corpuscles. This enzyme also destroyed the virus receptors in the extraembryonic cavities of chick embryos and prevented infection with influenza or mumps viruses. When the receptor-destroying substance was removed, the embryonic tissue recovered susceptibility, apparently because of regeneration of receptors, within twenty-four to forty-eight hours.

Of apparently somewhat different mechanism is the action of certain polysaccharides derived from bacteria on the mumps virus and the pneumonia virus of mice as studied by another group of workers at the Rockefeller Institute. These polysaccharides prevented agglutination of red blood corpuscles by influenza virus or mumps virus. They did not, however, prevent the adsorption of influenza virus to red blood corpuscles or inhibit the multiplication of this virus in chick embryos; they did prevent adsorption and multiplication of mumps virus under comparable conditions. In-

hibition of multiplication of the mumps virus in the allantoic cavity of chick embryos was obtained even when the polysaccharide was given as long as four days after the virus. Conversely, with the pneumonia virus of mice, several polysaccharides prevented multiplication in the lungs of mice but had no effect on the agglutination of red corpuscles by this virus. In these studies there was no evidence that the polysaccharides prevented attachment of the virus to the host cell or worked directly on the virus itself. Thus a series of observations based on the concept of blocking the attachment of virus to cells has led to the discovery of active inhibitory substances, but the mechanism of action of at least some of these appears to be different from that which was originally postulated.

The antibacterial action of certain chemotherapeutic agents is attributed to specific interference with an enzyme system or metabolic process of the bacterial cell. Success in the treatment of disease with such drugs depends on the lack of a correspondingly poisonous effect on the tissues of the infected animal. It is possible that the same process can occur regardless of whether the infecting agent multiplies (like bacteria) outside or (like viruses) inside cells, provided, in the case of viruses, that the chemotherapeutic agent can penetrate the cell membrane and that it is not destroyed inside the cell.

It is likely that the sulfonamides, acridines, and various antibiotics affect the same enzyme systems in the rickettsiae and chlamydozoaceae as they do in the bacteria. The infectious agents intermediate between viruses and bacteria may have lost many of the abilities for synthesis of their own essential nutrients and have thus become dependent for these on the host cell, but they probably have retained some of the bacterial enzymes inhibitable by chemotherapeutic agents. In the chlamydozoaceae, the mechanism of action of the sulfonamides and their reversal by the chemically related substance, *p*-aminobenzoic acid and its derivatives, seems to be identical with that found in bacteria. Other members of this same group of organisms are highly resistant to the action of sulfonamides in a way analogous to resistant bacteria.

In some of these intermediate infectious agents the enzyme systems may have undergone variation or mutation so that there have been evolved new systems which are related to, but not identical with, those found in bacteria. Since rickettsiae are not inhibited by sulfonamides but are inhibited by the closely related substance, sulfonamidobenzamidine, we may have here an example of such enzymic modification with a corresponding change in the structure of the substance required for inhibition. Other examples may be found in the acridine derivatives where modification in chemical structure influences the relative activities for protozoa bacteria, rickettsiae, and viruses.

The smaller viruses may be resistant to the antibacterial drugs, not so much as a result of their intracellular site of multiplication, but because they lack the vulnerable enzyme systems attacked by these substances and are dependent on other systems intimately associated with the synthetic and metabolic activities of the host cell. In viruses such as those of influenza, mumps, and rabies, which are one half to one third the size of the chlamydozoaceae, there may be still other enzyme systems which can be specifically inhibited by as yet undiscovered substances; or it is possible that other more active derivatives of acridine or hexamidine may be found. The smallest viruses may have been reduced by a retrograde evolutionary process to such a condition of metabolic dependence on the host cell that they have no enzymes and are little more than carriers of a hereditary pattern reproduced by some mysterious process in the cell. Is it possible to modify these enzymatic processes of the cell so as to present an unfavorable medium for the entrance and multiplication of the virus? Such a mechanism may be involved in the action of *p*-aminobenzoic acid on rickettsial infections, and in the action of polysaccharides on the pneumona virus of mice. It has also been found that deficiencies in certain vitamins seem to slow down the progress of infections with viruses like poliomyelitis in experimental animals, whereas other types of vitamin deficiency may favor the growth of viruses.

Further extension of the range of chemotherapy for the diseases caused by filtrable virus might be accomplished by additional empirical discoveries. It appears, however, that a satisfactory solution of the problem requires a knowledge more complete than we have at present of the fundamental life processes of cells.

THE NINETY-EIGHTH PRESIDENT OF THE AAAS

J. J. CHRISTENSEN*

Division of Plant Pathology and Botany, University of Minnesota, St. Paul.

A DISTINGUISHED national and international figure in his chosen field, plant pathology, Professor E. C. Stakman, of the University of Minnesota, is one of Minnesota's outstanding scholars and scientists. He holds a commanding position also in the broader fields of biology and agriculture, as attested by an ever-increasing demand for his services in various consultant and advisory capacities.

For many years Dr. Stakman has been an active Fellow of the American Association for the Advancement of Science and since 1947 has served as a member of its Executive Committee. He has contributed effective leadership to other scientific societies as well, particularly to The American Phytopathological Society, as president in 1922 and as committee chairman, including the War Board of American Plant Pathologists during World War I and the War Committee during part of World War II. His work in the National Research Council began in 1931 with membership in the Division of Biology and Agriculture and has included various offices; at present he is vice chairman of the Division and also chairman of the Committee on Aerobiology, which is engaged in a study of aerial dissemination of disease organisms between regions and between continents. In the American College of Allergy he serves on the Research Council. He is a member of the Advisory Committee on Biology and Medicine of the United States Atomic Energy Commission. Last fall he went to Japan on a scientific mission for the National Academy of Sciences. These and similar activities indicate his breadth of interest and experience in the agricultural and biological fields. What he has gained from this experience and from his other varied interests will be available for the furthering of the objectives of the AAAS when he becomes its ninety-eighth president on January 15.

Professor Stakman has made many outstanding contributions to science in the fields of plant pathology and mycology. His investigations are interesting not only because they contain much that is of practical importance to agriculture, but because of their elucidation of certain fundamental biological concepts. His pre-eminent researches have been on the genetic variation and the nature and extent of physiologic specialization in parasitic fungi, and in the epidemiology of stem rust. In his work on physiologic specialization he demonstrated for the first time that within a variety of a species of the stem rust fungus there were races, or strains, that looked alike but which behaved differently on different varieties of cereals and grasses. On the basis of this work he was awarded the Emil Christian Hansen gold medal and prize, in recognition of his accomplishments in the development of new ideas and of methods for investigating the rust problem. These contributions are applicable not only to a study of rust but to investigations of microorganisms in general.

Dr. Stakman's discoveries in physiologic specialization of fungi have been a tremendous stimulus to basic research in microbiology; they have changed our concept of a species if not more than, then as much as, any other single discovery in biology; they have modified our taxonomic concepts in the lower organisms; and they have revolutionized the method of developing resistant varieties of crop plants. They also are of great value in the production of antibiotic substances and in other industrial processes associated with microorganisms. A knowledge of specialization is of fundamental importance to sound procedure in justifying and in establishing plant quarantine regulations.

In spite of Dr. Stakman's zeal for research, he is primarily and outstandingly a teacher. He is an exponent of good teaching and has contributed materially to building up sound curricula for agricultural students at the University of Minnesota. In classrooms as well as in personal contact, he is constantly inspiring and stimulating students and fellow-workers to find out things by their own experimental investigations, to acquire a sound backlog of scientific facts determined by other workers, and to assimilate and organize demonstrated principles of science that are applicable to future problems. Lectures and seminars are usually not given in a formal, standard manner but are frequently interspersed with questions and with class discussion; often the philosophical implications of scientific data are considered. By devious means he

* The writer is greatly indebted to Miss Laura M. Hamilton for her assistance in the preparation of the manuscript.

stimulates discussion and will readily take either side of an argument in order to develop the ability of students to organize and synthesize facts. He is also a firm believer in laboratory work; to him there is no substitute for laboratory and practical experience in the biological sciences.

He also encourages the development of a broad cultural education; as one means of achieving this, he started a library, which includes the arts and the humanities, for the seminar room of the plant pathology building at Minnesota, to which students traditionally contribute upon their departure and to which Stakman honoraria are channeled. Although he is a strong believer in rigorous educational discipline, to most graduate students he is a real friend and counselor as well as an understanding teacher. He is at ease with students of all ages, understands their educational and personal problems, and participates in their sports, cultural activities, political and social arguments, and all intellectual discussions.

The remarkable fact is that Stakman's friendship with a student does not end when the student has completed his formal education and has earned his coveted degree. Many continue to bring their problems to him. They also like to bring their children to see him, and the children and "Stak" are soon fast friends. They quickly sense that he is a warm-hearted, human sort of person.

Students come from all parts of the world to study under him. Under his supervision more than 180 students have obtained higher degrees, and many others have come to Minnesota to take post-doctorate work. His former students are to be found in scientific laboratories of the United States and of the world. Some of his students have attained distinction in biological research; others have assumed executive positions in their government's agricultural organizations; a few have contributed to important international agricultural and educational programs, among them representatives to the Food and Agriculture Organization.

Dr. Stakman's enthusiasm for education and research has not been confined to the United States. He has traveled extensively on scientific missions in this and other countries and has been employed in an advisory capacity, during periods of leave from the University, in far-flung regions of the world. He has made several professional trips to Europe and has traveled around the world twice. In 1921 he was sent to Alaska by the U. S. Department of Agriculture to study rusts, and the next year to Europe to study the barberry, alternate host of black stem rust, in relation to the control of this destructive disease of wheat and other small-grain crops. For many years he has directed and personally made observations on stem rust in Mexico and the United States, particularly in the Great Plains area. In this work, Dr. Stakman and his colleagues were the first to use the airplane in determining the spore content of the air in studying the long-distance dissemination of plant-disease organisms. In the winter of 1930–31 he was guest lecturer at the University of Halle, Germany. He also has participated in international scientific congresses: as delegate to the first Pan-Pacific Science Congress in Australia in 1923; in the International Botanical Congress at Cambridge in 1930; and at Amsterdam in 1935, where he served as vice president of the section on microbiology.

While on leave in 1930, Professor Stakman was employed by the Firestone Plantations Company to investigate disease problems on their rubber plantations in Liberia; their present research laboratory there was set up on the basis of his recommendations. He also has firsthand knowledge of rubber growing in Ceylon and other areas of the Far East. During World War II, as part of the program of national defense in the Western Hemisphere, he was sent by the U. S. Department of Agriculture into northwestern South America to study native rubber in the jungles and the possibilities of developing *Hevea* rubber there.

In recent years Dr. Stakman has been active in the development of educational and research programs in Latin America. In 1941 he served as member of a survey commission appointed by the Rockefeller Foundation to study agricultural conditions and problems in Mexico, and in 1943 helped to implement the Foundation's program of agricultural improvement. As an extension of this work he has traveled recently in several Central and South American countries to study agricultural problems and to determine the status of education and research in the natural sciences. On the basis of his study, more extensive cooperative investigations have been instituted on plant-disease problems of international importance. He has been instrumental in encouraging Latin-American students to come to the United States for graduate work, and the first of them now have returned to important positions at home. Through his interest and encouragement, the Mexican Phytopathological Society has recently been organized.

As a result of this extensive experience, he has a sound knowledge and sympathetic understanding of the problems of this and other countries. And at the same time, as his students will testify,

he has an understanding sympathy for the problems of the individual in both Eastern and Western Hemispheres.

Elvin Charles was born in May 1885, in Algoma, Wisconsin, but has lived in Minnesota since early boyhood. He grew up in the village of Brownton, Minnesota, where he obtained his early education and farm experience. Stakman has not forgotten his farm life and enjoys relating boyhood experiences. He still has a genuine interest in rural life and is well versed in methods and types of farming, not only in Minnesota but in many other parts of the world.

In 1902 he graduated from Cleveland High School in St. Paul, and in the fall registered in the College of Science, Literature and the Arts of the University of Minnesota, majoring in biology and political science. He received the Bachelor of Arts degree in 1906 and was honored by election to Phi Beta Kappa. During the next two years he taught in Minnesota high schools at Red Wing and at Mankato, and in 1908–09 was superintendent of the school at Argyle. In 1909 he became a member of the University staff as an instructor in plant pathology and began his graduate work under his former botany professor, E. M. Freeman. Two years previously, Professor Freeman had established at St. Paul one of the first departments of plant pathology in the United States. In 1910, Stakman received the Master of Arts degree and, in 1913, the degree of Doctor of Philosophy. He now holds, in addition, an honorary degree of Doctor of Natural Sciences from the University of Halle-Wittenberg, Germany.

From 1913 to 1941, Dr. Stakman was head of the section of plant pathology; at present he is chief of the Division of Plant Pathology and Agricultural Botany. He also has been associated with the U. S. Department of Agriculture in various capacities since 1914. In 1918–19 he was pathologist in charge of the barberry-eradication campaign, in which he has long been interested. Since 1917 he has been Pathologist and Agent in the USDA in charge of cooperative investigations carried on with the University of Minnesota on stem rust and rust epidemiology. He also represents the University in directing cooperative relationships with Federal pathologists stationed at the Minnesota Experiment Station.

In 1917 he married Louise Jensen, of Minneapolis, who was then mycologist at Minnesota. They live near the St. Paul campus of the University.

Dr. Stakman has been honored by membership in the American Academy of Arts and Sciences, the American Philosophical Society, founded by Benjamin Franklin, and the Washington Academy of Arts and Sciences. He also holds, among others, honorary life membership in the Sydney (Australia) University Agricultural Society; he was made an honorary member of the Canadian Phytopathological Society in 1945, and a foreign member, in 1946, of L'Académie Royale d'Agriculture de Suède, founded in 1811. There is in addition a long list of professional societies of which he is a member.

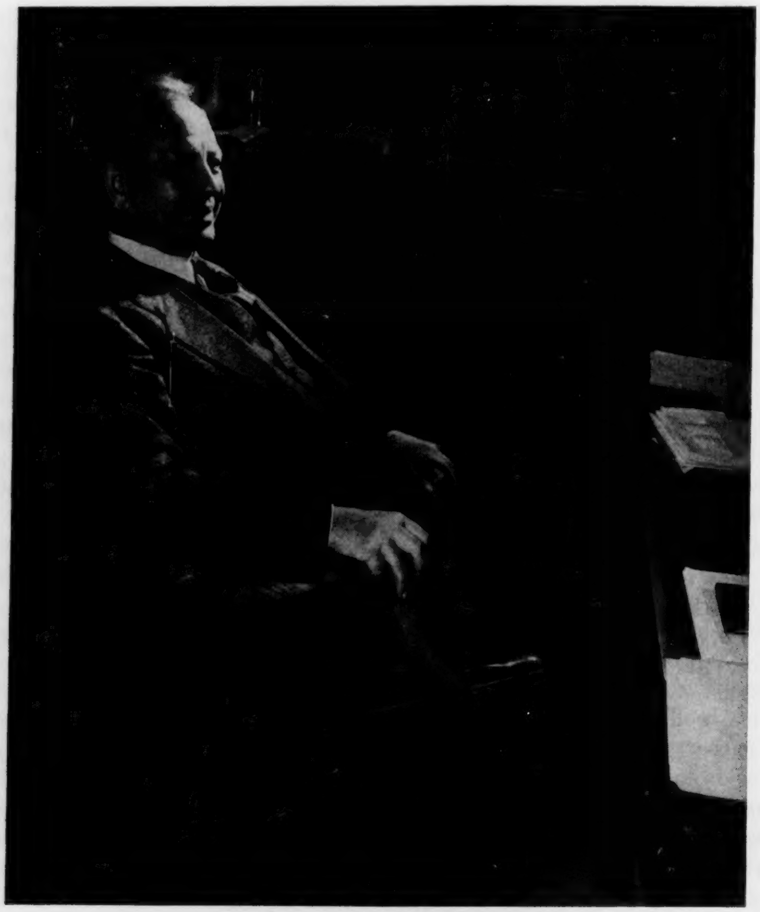
In both speaking and writing, Stakman expresses his thoughts with ease, clarity, and forcefulness. He is in demand as a speaker and last year traversed the United States in a lecture series sponsored by Sigma Xi, honorary scientific society. His publications include about 200 titles and embrace a wide range of researches. Many of the articles are classics in their field. Some are of a summary type, others have a general scientific and philosophical aspect. Most of his writings have been published in scientific journals or as bulletins, and some as chapters in biological books. He has pressing invitations to write books but has been too much occupied in research and in directing graduate students to undertake the compilation of texts. From 1925 to 1929, he was editor-in-chief of *Phytopathology*; before the war interrupted publication, he was American editor of *Phytopathologische Zeitschrift*; at present he serves on the editorial board of the *Annual Review of Microbiology*.

"The Big Chief," as the students call him, gets a tremendous amount of enjoyment from life, and to everything he does brings a stimulating vigor. He is genuinely interested in any subject whether it be history, politics, religion, football, or tobacco. (One of his hobbies is the collection of pipes.) Being a voracious reader and possessed of an incredibly retentive memory, he is well versed in a great many fields. He likes intellectual discussion; he has the unique ability to weave together apparently insignificant facts into a sound, logical argument, and will fight for what he considers a valid premise. Particularly fond of history, he will discuss at the least provocation diverse political situations with historical perspective and background.

He has always been very much interested in sports. He played baseball, tennis, and handball; and he coached baseball and football in high school. For years he was the driving force behind the plant pathology softball team at University Farm, first as a player, later as the coach. His motto was:

"Whether you work or play, do your very best." His team won the campus championship eighteen out of twenty years. He still has the happy faculty of relaxing completely, and he follows Minnesota football closely.

Dr. Stakman has probably contributed fully as much as an educator and propagandist for the advancement of science, culture, and civilization as he has as a professional scientist. He has a deep and abiding interest in education and in fundamental science, and he understands thoroughly their function and use. For years he has been a tireless fighter for the advancement of science, advocating more basic research, not necessarily for the pressing problems of today but to provide a reservoir of facts for future use. According to him, "Fundamental research over a long period of years . . . gives the most practical results."



ELVIN CHARLES STAKMAN

A BOTANICAL NONCONFORMIST*

RALPH E. CLELAND

Professor Cleland (Ph.D., Pennsylvania, 1919), who is head of the Department of Botany, Indiana University, delivered the address from which this article is taken as retiring president of the Botanical Society of America at the Dinner for All Botanists, Washington, September 11, 1948.

THE evening primrose, Oenothera, began its scientific career in the 1880s, more than 60 years ago. It chose as the stage upon which to make its debut, not its native haunts, but a foreign shore, having been transported from America to Europe, in all probability, in ships carrying ballast. Springing up on the ballast heaps of Europe, the evening primrose spread relatively unnoticed over the Continent. A few taxonomists examined it and attempted to classify it. A few florists picked out one or two of the showier kinds and introduced them into gardens in England and Germany. But the unique features of the evening primrose went unnoticed until 1886, when De Vries happened upon it. Since that time, the plant has received more than its share of attention.

The thing that first attracted De Vries' attention was the peculiar habit which the plant showed of giving rise on rare occasions to individuals very different from the common run of offspring. De Vries noted this in an abandoned field near Amsterdam, which the evening primroses had taken over. Lamarck's evening primrose, as De Vries called it, is a showy form, and the field where he found it must have been a place of beauty. But what attracted De Vries was the fact that, whereas most of the plants in the field were indistinguishable from one another, here and there a very different individual reared its head. Could these be what De Vries had been searching for, new species in the act of originating? Subsequent breeding experiments showed that such forms arose in each generation from *lamarckiana*, and that some, at least, of these new types bred true when selfed. De Vries was encouraged to think that he had a case where new "elementary species," as he called them, were springing full-fledged into existence, like Athena from the head of Zeus. Upon this assumption, and based largely upon Oenothera, De Vries developed his celebrated mutation theory of evolution.

Unfortunately it turned out, however, that there was much more to the behavior of the evening primrose than met the eye. These so-called mutations were not what De Vries thought they were, but were instead just one manifestation of the peculiar way in which Oenothera manipulates its chromosomes. At that time, of course, the part played by chromosomes in heredity was only dimly suspected.

It was not long before De Vries discovered that Oenothera is unusual in another respect. Not only does it throw occasional sports, it is also unique in its *normal* breeding behavior. Although De Vries made this discovery prior to 1900 when, along with Correns and Tschmermak, he unearthed Mendel's published work, he was already familiar from his own experiments with the essentials of what is now called Mendelian inheritance. He knew that homozygous or pure individuals breed true and that hybrids or heterozygous plants tend to produce splitting progenies. He therefore interpreted the fact that his races of Oenothera bred true (except for the rare mutations) to mean that they were pure. On this basis, two pure races crossed to each other should yield but one class of progeny. Instead, they often produced splitting progeny, and the hybrids thus produced, which should have yielded splitting progenies when selfed, often bred true. Thus De Vries was confronted with an anomalous situation. Other plants breed true when pure, and produce varied progeny when they are hybrid. Supposedly pure races of Oenothera, when crossed, often give splitting progenies, and undoubted hybrids often breed true. Thus, Oenothera seemed often to behave in a manner diametrically opposite to that of other plants. De Vries was unable fully to understand this behavior. He could not bring himself at first to admit that his races were heterozygous, as Renner insisted, for that would have called in question the significance of his so-called mutations. He tried to explain the formation of more than one kind of gamete in his supposedly pure races by assuming the presence of pangenes in a labile or unstable condition, which mutated, thus giving more than one kind of germ cell. This, however, failed to explain the behavior of Oenothera with complete satisfaction.

* The help of the Rockefeller Foundation and of Indiana University in all this work is gratefully acknowledged.

It remained for Renner to give the correct genetical interpretation. (Bartlett in this country hit on the same fundamental conception independently.) In so doing, he discovered uniqueness indeed in the genetical behavior of the evening primrose.

Briefly, what he found was this. Although the evening primrose has 14 chromosomes and should, therefore, according to normal Mendelian behavior, have 7 independent groups of genes, it frequently behaves as though it had but one pair of chromosomes and one linked group of genes. In most other organisms, the genes received from father and mother become shuffled when an individual produces reproductive cells, so that many combinations of paternal and maternal genes are possible among the germ cells produced. Thus, it should theoretically be possible for an evening primrose, with 7 pairs of chromosomes, to produce 128 different kinds of sperms and eggs. Instead, most races of Oenothera produce only 2 kinds, and these are identical with the sperm and egg which united to form the plants producing them. This is the behavior one would expect if it had received only 1 chromosome through the sperm and 1 through the egg. It receives, however, 7 from each gamete.

Renner also drew attention to two other peculiarities in these plants. The first was the presence of lethals (Fig. 1). Each set of genes received by a plant possesses a so-called lethal which makes it impossible for any individual to develop which has received this set of genes and this lethal through both sperm and egg. The lethal in one set of genes is different, however, from that found in the other set of genes in a plant. If we designate the set of genes received by a plant through the egg as alpha, and that received through the sperm as beta, the genetical constitution of the individual is then alpha beta. When this individual forms gametes, only two kinds are produced, those carrying alpha and those carrying beta. None of the offspring, however, can be alpha alpha or beta beta because of the lethals. Only alpha beta individuals can be formed if the line is inbred (and most evening primroses which show this behavior are habitually self-pollinated). Since in most races the alpha and beta sets of genes are very different from each other, we have the anomaly of a highly heterozygous race which breeds true, every individual in every generation being perforce alpha beta.

The other peculiarity analyzed by Renner was the fact that when two different races are crossed, the hybrids formed do not always behave as their parents do. They do not always produce just two kinds of gametes identical with the ones that united to form the hybrid. In other words, the genes which were all linked into a single group in each of the parents have now become partially unlinked in the hybrids. Strangely enough, if race 1 is crossed with races 2, 3, or 4, the genes transmitted to the F_1 hybrids are found to have become unlinked in different ways in the different hybrids, but always in the same way in the same hybrid, no matter how many times this hybrid is produced. Thus, one cross may produce a hybrid with two linkage groups, another may produce one with three groups, another one with four, and so on. Two genes may be linked in one hybrid but independent in another. In short, the single linkage group found in the parents has disintegrated in the hybrids, has disintegrated in different ways in different hybrids, but always disintegrates in the same way in the same hybrid.

This behavior, so beautifully analyzed by Renner, was quite unique and quite beyond explanation in terms of ordinary Mendelian behavior. In other organisms, linkage is due to the linked genes being bound up in the same chromosome, and they can become unlinked only by the disintegration in one way or another of the chromosomes in which they reside. It was, however, unthinkable that chromosomes that were intact in races would become altered structurally in hybrids between these races, and in different ways in different hybrids. Renner's analysis therefore revealed some very strange and unheard-of kinds of genetical behavior. No organism with such genetical behavior had ever been found before.

Since not even evening primroses are free from the laws of cause and effect, this strange behavior had to have an explanation, and this came to light when the chromosomes were critically examined.

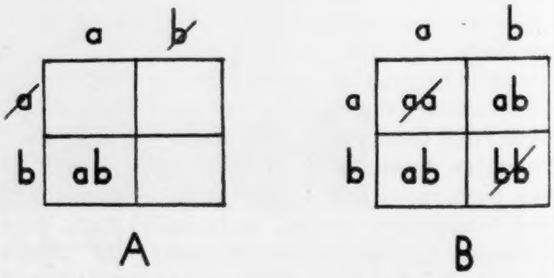


Fig. 1. Diagram showing the two kinds of balanced lethal situations in Oenothera. *A*, gametophytic lethals. One set of genes has a lethal which kills the male gametophyte, the other has a lethal which kills the female gametophyte. *B*, zygotic lethals. Each set of genes has a lethal which kills any zygote receiving this set in double dose. Either kind of balanced lethal situation makes it impossible for an individual to exist which has received the same set of genes through sperm and egg.

It was at this point that the author entered the picture with the discovery that chromosome behavior in meiosis in Oenothera is as unique as its genetical behavior, and that this unique chromosome behavior is of such a nature as to explain very neatly the peculiar genetical behavior analyzed by Renner.

Briefly, these peculiarities in chromosome behavior are as follows: In the reduction divisions which occur at the formation of spores in anthers and ovules, the chromosomes fail to pair as they do in other organisms. Instead, they become associated end to end into a closed circle, all 14 chromosomes being united into a single circle in most races (Fig. 2, *A*). At the first reduction metaphase, the circle remains intact and the chromosomes orient themselves in the middle of the spindle in such a way that adjacent chromosomes become attracted toward, and later move to, opposite poles (Fig. 2, *B*).

This behavior, which was unknown until found in Oenothera, furnishes the basis for the breeding peculiarities that Renner analyzed. Obviously, what happens is that a given chromosome, descended, let us say, through the sperm, synapses at one end with a chromosome derived from the egg and at the other end with a different egg-derived chromosome. In other words, it is homologous at one end with one chromosome and at the other end with another chromosome, and so it synapses at its two ends with two different chromosomes. This is true of all the chromosomes in the circle. The result of this setup is that chromosomes of paternal and maternal derivation alternate with each other in the circle, the force holding them together being the ordinary synaptic force.

But if paternal and maternal chromosomes alternate in the circle, and if adjacent chromosomes go to opposite poles of the spindle, obviously all paternally derived chromosomes will go to one pole and all maternally derived choromosomes will go to the other pole (Fig. 3). Then all paternally derived genes will go to one pole and into the same germ cells, and all maternally derived genes will go to the other pole and into other germ cells. The result will be the same as though all paternal genes were in one chromosome and all maternal genes in a single corresponding chromosome. Thus, although there are 14 chromosomes, the plant behaves normally as though it had but one pair of chromosomes, producing only two kinds of germ cells—one kind being identical with the sperm, the other with the egg that united to produce the plant. Since each set of genes has a zygotic lethal and cannot as a result exist in double dose, and since the plant is generally self-pollinated, thus excluding as a rule sets of genes from other plants, all the progeny of a plant will normally be exactly like itself, recombining the same two sets of genes which this plant itself received and which it transmits intact to the next generation. This will continue indefinitely, the same two kinds of sperm and egg being produced in each generation. Each set of genes is transmitted from generation to generation intact and indefinitely. Each is a continuing entity. We call it a Renner complex. The plant breeds true, therefore, but not because it is homozygous—in fact, in all such races the two sets of genes are noticeably to strikingly different genetically.

But how does this anomalous chromosome behavior explain the bizarre finding of Renner that the sets of genes which were in effect linked in the parents are not necessarily linked in hybrids between races, that they become unlinked in different ways in different hybrids? The obvious suggestion is that the large circle of chromosomes no longer exists in such hybrids, that different arrangements of chromosomes exist in different hybrids. Theoretically, two sets of 7 chromosomes in Oenothera can become associated in 15 different ways, forming configurations ranging from ⨀ 14 to 7 pairs (Table 1). If a hybrid has its chromosomes arranged into two independent circles, its genes might be expected to act as though they were in two independent linkage groups. If there are three circles present, the genes should act as though they were distributed among three linkage groups and so on. It was easy to test

A

B

Fig 2. *A*, diakinesis in pollen mother cell, showing circle of 14. *B*, first meiotic metaphase, with circle of 14 intact, adjacent chromosomes going to opposite poles.

Fig. 3. Diagram to show that the meiotic mechanism produces gametes that are identical with the gametes which united to form the plant. Alternation of paternal and maternal chromosomes in the chain, and the separation of adjacent chromosomes, bring about this result.

this suggestion by making crosses between races and examining the hybrids to see how their chromosomes behaved in relation to the way their genes behaved. I was fortunate to have the opportunity of spending two summers and the intervening winter with Renner and Oehlkers, subjecting this hypothesis to extensive tests. The chromosome behavior of many hybrids was examined, and these hybrids were then subjected to breeding tests to determine as far as possible the number of genetic linkage groups present. The results were striking. Those hybrids which had a single large circle bred essentially true, showing that their genes were linked into a single group. Hybrids with several smaller circles or a mixture of circles and pairs produced splitting progenies, the complexity of this splitting increasing as the number of independent circles or pairs increased. Thus the phenomenon which Renner observed in hybrids was found to follow a very simple rule; namely, the number of linkage groups is equal to the number of chromosome groups, whether these be pairs or circles. Many hybrids between races have been studied since this initial investigation was made. All 15 arrangements into circles or pairs have been found over and over again among these hybrids, and their genetic behavior, wherever tested, has been what might be expected on the basis of this hypothesis.

All of this, however, raises further questions. Why do the Oenothera chromosomes behave in this peculiar way? Why do they not pair normally as they do in other organisms? In some races of Oenothera, particularly in some subgenera, they do behave normally, but the majority of the races throughout most of the range have ⊙ 14 as their chromosome configuration. Why is this peculiar behavior found in the races, and why are different chromosome arrangements found in different hybrids?

Belling was the first to offer a fruitful suggestion in this connection. Finding a circle of 4 chromosomes in a hybrid between two races of Datura, he suggested that one of the parental races involved had suffered an exchange of segments (reciprocal translocation) between two nonhomologous chromosomes (Fig. 4). From one parent the hybrid had received only normal chromosomes, but from the other parent it had received a pair of translocated chromosomes. When germ cell formation took place in this hybrid, and homologous regions synapsed, the translocated chromosomes gave ⊙ 4 with the untranslocated ones. Belling suggested that reciprocal translocations such as this might be the explanation of the Oenothera behavior, large circles being formed as the result of a series of translocations, one following the other.

There is no time to go into the way in which it was proved that Belling's suggestion was indeed valid for Oenothera. Suffice it to say that the work of a number of different investigators has shown beyond doubt that circles have come into existence in Oenothera as a result of reciprocal translocations, the large circles being the result of series of successive translocations. Such translocations are known in other organisms, especially in plants, but Oenothera is unique among organisms in the extent to which it has experienced this sort of exchange. A few other genera are known in which a single species has had sufficient background of interchange to have developed a circle incorporating all its chromosomes. No other group is known, however, in which the great bulk of the races existing in nature have had

TABLE 1

POSSIBLE ARRANGEMENTS OF 14 CHROMOSOMES INTO CIRCLES AND PAIRS

⊙ 14
⊙ 10, ⊙ 4
⊙ 8, ⊙ 6
⊙ 6, ⊙ 4, ⊙ 4
⊙ 12, 1 pair
⊙ 8, ⊙ 4, 1 pair
⊙ 6, ⊙ 6, 1 pair
⊙ 4, ⊙ 4, ⊙ 4, 1 pair
⊙ 10, 2 pairs
⊙ 6, ⊙ 4, 2 pairs
⊙ 8, 3 pairs
⊙ 4, ⊙ 4, 3 pairs
⊙ 6, 4 pairs
⊙ 4, 5 pairs
7 pairs

such a background. Oenothera has gone to an extreme in this regard.

As a result, the Oenothera population possesses a structure which is in many respects unparalleled among plants or animals. Throughout most of the range, the population consists of a multitude of races with ⊙ 14, more or less isolated from one another because of the self-pollinating habit, breeding true because of the large circles and the lethals which allow only one genetic combination to exist in each race, this same genetic combination appearing over and over again in practically every individual of every generation. The circle of 14 chromosomes present in each race is composed of two sets of chromosomes which have had different histories of interchange and are therefore entirely unlike each other in the arrangement of their segments. Not a single chromosome of one set is completely homologous with any chromosome of the other set. To make this clear, let us designate each chromosome by two numbers connected with a dot, the numbers representing end segments of the chromosome. One set of chromosomes might be designated as follows: 1·2 3·4 5·6 7·8 9·10 11·12 13·14. The other set of chromosomes associated with it to form ⊙ 14 will have an entirely different arrangement of segments, let us say, 2·3 4·5 6·7 8·9 10·11 12·13 14·1. Synapsis of corresponding segments will produce ⊙ 14. There are of course many arrangements of segments which will give ⊙ 14 with each other, and many different arrangements exist among the hundreds or thousands of races bearing ⊙ 14.

How has such a situation arisen? It is logical to suppose that once upon a time the ancestors of the Oenotheras were all normally behaving plants, with paired chromosomes. Then translocations began to take place and circles began to arise, at first small, then larger and larger. Did this happen within races, each race with ⊙ 14 thus being the result of a separate and distinct evolutionary process? The evidence indicates that this was not the case but that different interchanges occurred in different strains, and then by occasional hybridization, and by interchanges within or between the complexes of the new hybrid strains, new segmental arrangements were formed, many of which by further hybridization came to be associated with different complexes to form different races, a given arrangement thus being present in more than one race. As a result, we find that many different segmental arrangements exist, and many combinations of complexes are to be found in different races. We also find that, although the two associated complexes in a race are usually dissimilar in segmental arrangement, it is common to find complexes in different races which are similar or even identical in the arrangement of their segments. All of this suggests that when we find complexes in different races which are similar or identical segmentally, they are phylogenetically closely related and have had a recent common ancestor. This gives us a clue to the evolutionary relationships that exist in the group.

Using this clue, we have tried to determine where the relationships lie among the North American races of the subgenus Euoenothera. This enormous group, the largest of the 15 subgenera of Oenothera, is the one that includes most of the races studied by De Vries and Renner. It is also a very difficult group taxonomically. I would like to show how we have tried to ferret out these relationships and very briefly to indicate what sort of situation we have found. Our method has been to take two races and cross them. If the complexes of these races are closely related, their segmental arrangements will be similar or identical, and when they are combined in the hybrid they will give mostly or entirely paired chromosomes with each other. If the complexes of the two races are unrelated, they will have unlike arrange-

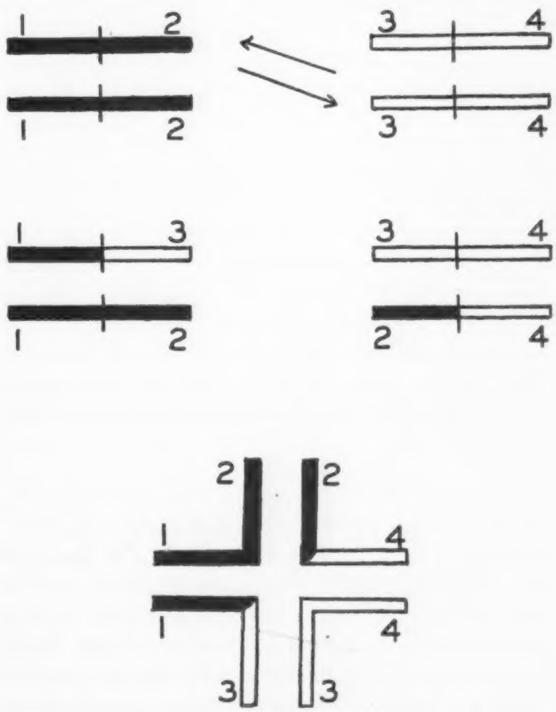


Fig. 4. Diagram illustrating reciprocal translocation. *Above*, two chromosome pairs, their ends numbered 1 and 2, 3, and 4, respectively. *Middle*, arrangement of segments following interchange of ends 2 and 3. *Below*, result of synapsis following interchange. A circle of 4 will result, instead of separate pairs.

ments of segments and will give large circles with each other when combined. By making all sorts of crosses, one can discover in this way many cases of relationship.

We can go farther than this and take one complex as a standard and determine the segmental arrangements of other complexes in terms of this standard. We can then take each of the arrangements thus determined and compare it with the primitive arrangement, and in many cases can see what interchanges have taken place to give rise to these arrangements. We can get a rather clear idea in this way of how they have evolved and how they are related both to the original and to each other.

As a result of our analysis of segmental arrangements, we find that the hundreds of races of the North American Euoenotheras tend to fall rather sharply into a small number of larger groupings, of which we have so far recognized seven. Each of these groupings is composed of races that have much in common. Phenotypically they are much alike, geographically they occupy near-by areas, and their complexes show a close relationship in segmental arrangement to one another. On the other hand, each of the large groupings is quite different from the others in cytogenetic behavior, and in most cases it differs from them also in phenotype and in geographical distribution. These groupings, therefore, have many of the features of species, and where they can be recognized phenotypically (in a few cases they cannot), it is probable that they should be accepted as species.

I shall not try to describe these groupings at this time. Suffice it to emphasize that the North American Euoenotheras are made up of multitudinous true-breeding and relatively isolated races and that these races fall rather neatly for the most part into natural groupings that in some cases may prove sufficiently distinct to deserve acceptance as species.

There is one other aspect of the behavior of the evening primrose that I should like to mention. Oenothera is a shining example of the fact that seemingly deleterious alterations of the hereditary mechanism may sometimes and under certain circumstances be turned to good advantage in the course of evolution. Oenothera has suffered several kinds of alteration or mutation, any one of which by itself might have placed it at a disadvantage in the struggle for existence. By combining all these apparently disadvantageous mutations into one plant, however, they have been turned to advantage in the case of Oenothera and have given it greater survival value than it probably would have had if these changes had not taken place.

The first seemingly deleterious type of alteration to appear was the one we call reciprocal translocation. In most organisms, translocations are disadvantageous, since they lead to frequent failure of the chromosomes to segregate properly in the reduction divisions, resulting in sterile germ cells. Oenothera, however, seems to have overcome this danger quite neatly. It fortunately began with a set of chromosomes all of which were of the same size, with spindle attachment regions placed in the center of the chromosome. It seems to have developed a technique by which, as a rule, the breaks occur close to the spindle attachment point. Thus, all the exchanges whose products have survived, and all which have been observed to occur naturally, have resulted in equal interchange segments, and have therefore produced little alteration in the size or structure of the chromosome. When the chromosomes in a circle are all equal in length and have median spindle attachments, the amount of irregularity in the separation of the chromosomes in meiosis is reduced to a low level and the resultant sterility is negligible. This is the case in Oenothera which, because it has not suffered seriously from the occurrence of interchanges, has had a tendency to retain the interchanges that have occurred, thus developing in the population a large amount of heterogeneity from the standpoint of segmental arrangement.

A second type of mutation developed by Oenothera that would seem to constitute a handicap was the production of lethals. Lethals produce serious sterility. One kind, the gametophytic lethal, kills every sperm it enters, or every egg, as the case may be; another kind, the zygotic lethal, kills every zygote into which it enters through both sperm and egg. A pair of zygotic lethals, one in each complex of genes, will kill 50 percent of the offspring. The presence of such a lethal is therefore a serious matter, and lethals must have been a great handicap in the earlier stages of Oenothera evolution. But the time came when Oenothera was able, through the increasing heterogeneity in segmental arrangement, to bring together sets of chromosomes which were entirely unlike each other and which therefore gave ⨀ 14 with each other. When such situations began to arise, Oenothera was at last able to derive an advantage from this erstwhile handicap, the presence of lethals, as the following will show. If a pair of balanced lethals is present in a single pair of chromosomes, it will ensure the heterozygosity of this pair, since it is impossible for either chromosome of the pair to exist in double dose. Hetero-

zygosity is advantageous in that it contributes toward hybrid vigor. It would, therefore, be of advantage if all the chromosomes could be made permanently heterozygous and thus bring about a maximum of hybrid vigor. But a pair of balanced lethals cuts fertility down to 50 percent. If balanced lethals were to appear in all the pairs of chromosomes, the increased hybrid vigor thus achieved would be offset by a tremendous reduction in fertility. The first pair of lethals would cut the fertility down to 50 percent; a second pair would cut the remaining fertility down another 50 percent; and so on. Obviously, an increase of this sort in the number of lethals would result in extinction. When a $\odot$ 14 shows up, however, a single pair of lethals will ensure the heterozygosity of the whole group of chromosomes. Since all the chromosomes of paternal origin in the circle go into the same germ cell, they all accompany any lethal present in one of these chromosomes and the whole set of chromosomes is prevented from existing in homozygous condition by this one lethal. A single pair of lethals, therefore, will ensure the heterozygosity of all the chromosomes. Thus, at the expense of only 50 percent reduction in fertility, the heterozygosity of all the chromosomes—consequently the maximum in hybrid vigor—is ensured.

But it will be argued that a loss of 50 percent in fertility is a serious loss, and so it is. It is questionable whether the hybrid vigor gained thereby would have been worth what it cost if it had not been for a third, seemingly unfortunate, mutation that occurred. This mutation was a reduction in length of the style, bringing the stigma down to the level of the anther. As a result, hours before the flower opens, the flower is self-pollinated and little opportunity is thus afforded for pollen from other sources to function. Self-pollination in general is a bad thing, for it tends to bring about homozygosity and eliminate hybrid vigor. In this case, however, its bad effect is prevented. The lethals will not allow homozygosity to occur. On the other hand, self-pollination has a positively good effect, since it ensures a much heavier pollination than would be likely if the plant were pollinated by insects, thus helping to overcome the sterilizing effect of the lethals. As a result of this fortunate combination of what would otherwise be unfortunate characters, both the lethals and self-pollination are prevented from having any harmful effect and are allowed to produce only their beneficial effects. The lethals ensure maximum hybrid vigor, and their sterilizing effect is largely balanced by the richness of pollination brought about by the self-pollinating habit. Self-pollination in turn cannot bring about reduction in hybrid vigor because the lethals prevent this action. The lethals furthermore are enabled to accomplish their desirable function only because of the presence of the large circles resulting from successive reciprocal translocations.

We thus have the unusual picture presented to us of three different kinds of alteration, each of which by itself might have proved a handicap in the struggle for existence. By developing a technique of interchange, however, which does not give rise to much sterility, and then combining the results of interchange with the presence of potentially deleterious lethals and self-pollination, the genus has achieved a combination of characters that has given it great survival value. It is interesting to note that the genus has spread from the southern tip of South America to the far reaches of northern Canada, and from the Pacific to the Atlantic, and that in general the sections of the genus that have ranged the farthest and are the most numerous are the ones with large circles, lethals, and self-pollination. Thus the genus presents us with a unique example of the way in which it is possible, in the evolutionary process, for apparently deleterious characters to combine in such a way that together they give to the plant increased survival value.

Space does not permit a deeper excursion into the intricacies of Oenothera cytogenetics. Enough has been said, however, to give some idea of the the degree to which Oenothera differs from ordinary organisms. Taking a number of untoward alterations in its hereditary make-up and combining them in such a way that they have increased rather than decreased the survival value of the plants possessing them, the evening primrose has developed a type of cytogenetic behavior and a kind of population structure that so far as known are quite without parallel.

It has been said that one should cherish his exceptions. If this is true, then Oenothera is especially to be prized. Originally regarded as one of the major puzzles among the higher organisms, it now stands forth as one of the most instructive exceptions to ordinary cytogenetic behavior and as one of the most interesting cases of unusual evolutionary development.

A SURVEY OF JAPANESE SCIENCE

HARRY C. KELLY

A graduate of MIT, Dr. Kelly (Ph.D., 1936) has been Deputy Chief, Scientific and Technical Division, and Chief of Special Projects Unit, General Headquarters of the Supreme Commander for the Allied Powers (Japan), since 1946. He had previously done research and taught at Lehigh University, Montana State College, and at the Radiation Laboratory, Massachusetts Institute of Technology.

A SCIENTIST'S first impression of Japan is of many technological developments copied from Western countries, and of Japanese scientists and technologists whose chief aim seems to be to get Western scientific and technical books to overcome the effects of their isolation during the war.

After several years of observation, this first impression still holds true. It also becomes evident that the Japanese are unquestionably the technical leaders of the Far East, that they have made world contributions to the advancement of mathematics, theoretical physics, and chemistry; that in fields indigenous to Japan, such as agriculture, fisheries, and sericulture, the Japanese show great skill in practical methods and the application of recent scientific results; and that in some subdivisions of these indigenous fields, Japan may lead the world.

The word "science" as used for organizational purposes in Japan includes all fields of learning, such as law and literature and the natural and social sciences. The modern forms of Western learning were superimposed on earlier Chinese art, literature, and medicine, so that, to the Japanese, scientific research means any kind of advanced intellectual activity.

The primary purpose of this account is to relate the changes made in the scientific structure in Japan during the present occupation—an occupation whose chief functions seem to be to make the occupation unnecessary, to remove war potential, and to allow Japan to achieve a stable economy in a democratic manner so that she can assume her responsibilities in promoting world peace. In a nation of about eighty million people, increasing at the rate of approximately one million per year in an area about that of California—a country that can raise only about 80 per cent of its food requirements and is short of natural resources—the responsibility is frightening indeed.

The occupation has adopted the sound policy that this responsibility is Japan's. Science and technology are considered essential to recovery, and fortunately many Japanese scientists and engineers recognize this responsibility and the necessity for reorganization of some of the national scientific bodies, which, because of a somewhat feudalistic influence, were incapable of attacking the new problems forced upon them. In general, the attitude of the occupation authorities is to ensure that the Japanese scientists have an opportunity of assuming their responsibilities, and that technical policies shall be determined by competent and representative scientists.

The three national bodies of science are the Imperial Academy of Science, the National Research Council, and the Japanese Society for the Promotion of Science. The reorganization of these bodies was one of the important problems facing the scientists. One of the problems was to devise a method whereby a body of the best scientific talent of the country, elected by competent fellow-scientists, could be used to guide and coordinate the development of science and technology in Japan and at the same time provide a sound democratic governmental structure. The Japanese have offered a solution in an elected body of scientists called the Science Council of Japan, and a governmental committee called the Scientific and Technical Administrative Commission.

The present Japanese culture began with the introduction of Chinese learning in the sixth century A.D. This exchange of learning was brought about by scholars, technicians, and Buddhist priests who came from China and Korea, and later by Japanese students who visited China. The introduction to Chinese culture had such a profound influence that it almost completely overshadowed the native culture. This was especially true in science and technology. The Japanese learned Chinese astronomy (calendar making), mathematics, medicine, sericulture, weaving, paper making, metallurgy, pottery, shipbuilding, architecture, surveying, etc.

The custom of sending envoys to China ended in A.D. 894. Most Japanese feel that this had no serious effect on developments in Japan. They believe that significant elements of the culture had already been assimilated and that China did not achieve anything outstanding during this period. In this early contact with Chinese culture, Japan was merely an imitator, but gradually her own characteristic culture began to emerge.

Early Japanese culture flourished in the eighth to the eleventh centuries; then came a 400-year dark age—the years of civil war. The intervals between wars were not long enough for any significant advancement. There were, however, some minor war-related technical developments, such as sword and armor making and castle building.

The arrival of the Portuguese in Tanegashima (a small island south of Kyushu) in 1543, with the introduction of the musket, is popularly regarded as the first encounter with Europeans. The Portuguese traders were followed by the missionary St. Francis Xavier in 1549. At that time Japan was still in a state of turbulence, but national peace was finally established in 1585. From 1592 to 1598, however, the country was at war with Korea. Japan lost the war, but again came into contact with Chinese and Korean culture—a culture that had made some advancement during the 700 years of Japan's isolation. The defeated army brought newly published Chinese books, as well as Chinese technicians, back to Japan. This again gave new impetus to the development of Japanese technology.

In the year 1603 the Tokugawa Shogunate came into power. This regime lasted until 1868, and in the 265 years of the Shogunate rule the Japanese enjoyed national peace. During this period, great advancements in science and technology were made.

At the time of the introduction of European culture into Japan in the sixteenth century some scientific and technical knowledge was passed on by Christian missionaries. The Japanese listened to the missionaries with great interest, and a few people from all classes were converted to the new creed. Later the government began to see danger in the propagation of Christianity, fearing that Spain and Portugal were using religion as a means of invasion, and Hideyoshi prohibited the teaching of Christianity. This policy was also followed and most strictly enforced by the Tokugawa government, finally resulting in Japan's closing its doors to all European books, including Chinese translations of such books. This prohibition was announced in 1630 and marked a new era of isolation from European culture. A limited amount of trading was allowed with the Dutch at the Port of Nagasaki. Even here caution was used, for the only contact with the traders was through interpreters (who were allowed to learn to speak, but not to read, Dutch).

As time went on, special features of European culture came to be more keenly appreciated from the material imported by the Dutch. On the advice of scholars of the period, Shogun Yoshimune in 1720 removed the ban on foreign books other than those dealing with religion. (This so-called period of "Dutch learning" lasted nearly 150 years, until the end of the Shogunate government.) Translations of Dutch books were soon published, books on medicine and astronomy far outnumbering other technical books. Very few if any books on physics and chemistry were translated, and, since facilities for experimentation were lacking, little progress was made along these lines.

Mathematics, however, seems to have been in a quite different category. Starting with the knowledge obtained from some Chinese books on mathematics, the Japanese made unique advancements in the seventeenth and eighteenth centuries. These studies comprised algebra, geometry, and calculus, and there is some evidence that a few of their independent discoveries predate the equivalents in European mathematics. Japanese mathematicians continued their isolated study even after the importation of European books in 1720 and for a short period after the Meiji Restoration in 1868. Mostly owing to the inferiority of Japanese notation, however, the Japanese finally replaced their methods with those from Europe. It is quite probable that the mental training the Japanese received in their development of mathematics was extremely valuable in understanding European science in succeeding years.

Just as in mathematics, there was some independent development in Japanese medicine. The introduction of Western medicine by the Dutch paved the way for the importation of other Western sciences. Japanese medicine was not investigated in the light of modern theory; the investigation would probably be most difficult, not only because of terminology but also because of the mode of analysis.

With the surrender of the Shogunate in 1868 to the Mikado, Japan fully opened her doors to foreigners, and Japanese people were allowed to go abroad. The Japanese put great effort into the study of European and American science and technology to overcome the limitations imposed by long periods of isolation. In the engineering

fields Japan is still in the imitating phase, but her accomplishments in the theoretical sciences of mathematics, theoretical physics, and chemistry, her work in sericulture and agriculture, indicate the contributions that the Orient can make to Western culture.

NATIONAL SCIENTIFIC SOCIETIES

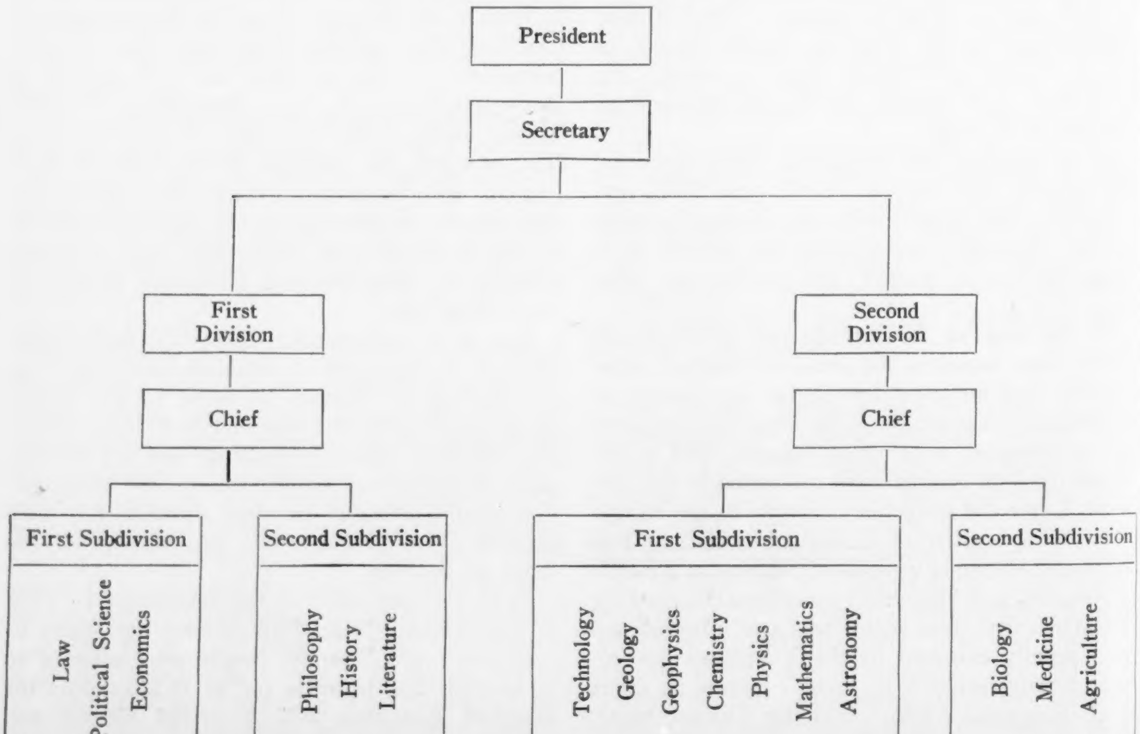
The Tokyo Academy, the predecessor of the present Imperial Academy, was founded as a governmental organization in 1879 by the Minister of Education upon the suggestion of Dr. David Murray, an American adviser. The purpose of the new organization was to advise on educational matters and to promote arts and science. The maximum number of members was 40; the Minister of Education appointed the first 7, and the Academy was to select other members with the approval of the Minister. The Academy initiated *The Tokyo Academy Magazine* on its establishment.

In 1906 the Academy was reorganized, and the Imperial Academy of Science came into existence on promulgation of Imperial Ordinance No. 149. The statutes provided that the Academy was to be under the supervision of the Minister of Education and was to have for its aims the development of learning and the promotion of culture. It was a self-perpetuating body. The members were appointed by the Emperor from among men of learning, on the recommendation of the Academy. It was divided into two sections: the Cultural and Social Science Group and the Pure and Applied Natural Science Group (Table 1). The Academy could undertake research projects in cooperation with a foreign scientific organization upon approval of the Minister of Education, and it could become a member of such a foreign organization. The Academy was also allowed to elect distinguished foreign scientists as members. To date, however, only 5 foreign members have been elected, and none of these is living at present. The members of the Academy originally had the right of selection of four members to seats in the House of Peers. This right has now been abrogated under the new Constitution.

In June 1919, the Imperial Academy submitted a memorial to the Minister of Education advising the establishment of a National Research Council. This was created in 1920 by Imperial Ordi-

TABLE 1

Japanese Imperial Academy



nance No. 297 and was placed under the jurisdiction of the Minister of Education. It had for its aims the unification, promotion, and encouragement of scientific research and its applications. "Science," as shown in Table 2, included jurisprudence and literature. A section on aeronautical engineering was eliminated at the end of World War II, since a Far Eastern Commission directive prohibits civil and commercial aviation.

An ineffectiveness in the Academy and the Council in the accomplishment of their aims is evident, for in January 1931 one hundred influential people representing higher learned circles met at the Imperial Academy Building and resolved to start an earnest campaign to establish a more effective organization for the promotion of scientific research.

After many committee meetings, in May 1933, the Japanese Society for the Promotion of Science was inaugurated. The Society is a juridical person with the objectives of promoting scientific research and forwarding its practical application, thus contributing to the advancement of culture, the development of industry and national defense, and the enhancement of national prosperity and human welfare. Although a juridical person is not a governmental body, the Society to all intents and purposes behaved as though it were.

In addition to these three important national bodies, there are many professional societies, such as the Japanese Physical, Chemical, Biological Economic, Electrical Engineering, and Mechanical Engineering Societies. These, however, are nongovernmental in character and perform their functions of encouragement of particular fields by discussion and publications. No attempt has been made during the occupation to interfere with them; rather, encouragement has been given to form societies in other specialties, in the hope that the professional societies, whose membership was open to any qualified candidate, would form the nucleus of a more representative and effective national council of science.

SCIENTIFIC ORGANIZATIONS DURING THE WAR

During the war, Japanese science made no noteworthy successful or original contributions, but concentrated almost entirely on discovering substitutes or making minor improvements. This failure was probably due mainly to three factors: distrust by the military of scientists who had been trained or had traveled abroad (these scientists are among Japan's best); poor coordination among the scientists themselves, and the nearly complete lack of coordination between the Japanese Army and Navy, which amounted almost to antagonism; and, probably the most important, the overwhelming might of the scientific and technical developments of the Allies, which had a most demoralizing effect on Japanese research.

The Japanese had very little contact with German science during the war. A few instruments, such as radar, ultrasonic, and infrared apparatus were introduced, but these were not the latest models and some were of prewar origin. There was some preliminary research on such items as proximity fuses, homing bombs, and jet propulsion, but because of poor technique and organizational difficulties not much progress was made. The amount of pure research, however, especially in mathematics and theoretical physics and chemistry, which went on during the war, is surprising. As an example, the theoretical work in nuclear physics kept very nearly abreast of developments in the rest of the world.

The predominant active organ during the war was the Board of Technology, which was under the direct control of the Premier. The Board was a kind of centralized administrative planning organization and placed particular emphasis on aeronautical research. Under the Board, the Council for the Mobilization of Science functioned as an operating agency and assigned research projects to compulsorily nominated personnel. Assignments for volunteers were made through the Japan Society of Technologists. Encouragement for inventive effort was provided by the Imperial Association for the Promotion of Invention. Organizations such as the Science Mobilization Association, the All-Japan Union of Learned Societies, the Japan Society for Aeronautical Techniques, and the National Research Council entered into activities to further the policies directed by the Board of Technology.

Many smaller organizations and committees were formed, such as the Science Neighborhood Groups. The Japanese natural tendency toward theoretical interest and discussions led them to attempt the solution of many problems in committee rather than in the laboratory.

In the encouragement of reorganization of the national scientific bodies by the occupation authorities, this weakness in war potential of the old organizations was not taken lightly. On one hand was the policy that science and technology were to be encouraged, since they were considered essential to economic recovery; on the other, a powerful governmental scientific body might well

serve as the genesis of a new technical General Staff with warlike aims. It was finally decided that if, under the new Constitution, an elected nongovernmental council of scientists were formed, it would be most likely to accept its responsibilities in promoting world peace.

RESEARCH DURING THE OCCUPATION

Research was very nearly at a standstill at the beginning of the occupation. The most important factors were the pressing problem of food and the welfare of the scientists' families. In addition, there was the uncertainty among the Japanese as to what research was to be allowed and what laboratories were to be affected by reparations and deconcentration of the Zaibatsu companies.

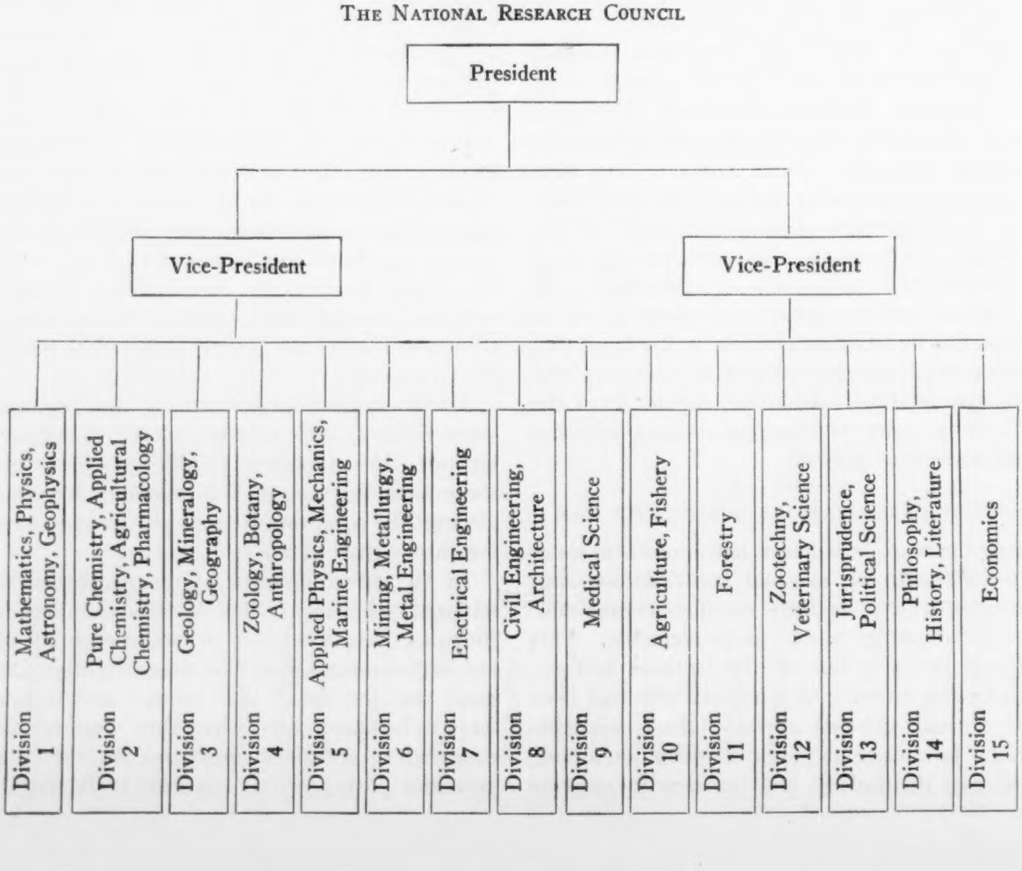
Compared with the rest of Japan, the laboratories survived the bombings fairly well. Most damage was inflicted on industrial laboratories, although a few universities also suffered severely. The laboratories at the University of Literature and Science in Hiroshima and Nagoya University were probably the most severely damaged university laboratories. The universities at Osaka, Sendai, and Waseda and Keio in Tokyo sustained some damage to their laboratories, but many others, such as the Imperial Universities of Tokyo, Kyoto, Kyushu, and Hokkaido, suffered no destruction.

The invading troops did some damage to scientific apparatus, but, considering the circumstances, the damage was not great. Much publicity has been given to the destruction of the four cyclotrons in Japan, but this destruction probably worked in the best interests of Japanese scientists, for it drew attention to the problem of the place of science in society and the necessity for adopting a just attitude toward it.

The first several months of the occupation were taken up with surveillance of Japanese laboratories and a kind of mutual taking of measure with the scientists. After this initial period it became evident that rather than discourage research it was necessary to encourage the scientists to resume their activities, especially where these involved the technical problems associated with food, clothing, shelter, health, and export. It became a policy of the occupation that science and technology were necessary to the economic recovery of Japan, and

TABLE 2

THE NATIONAL RESEARCH COUNCIL



Japanese scientists were encouraged to assume these responsibilities.

The limitations on research were based on the Potsdam Declaration and Far Eastern Commission directives. The Japanese were informed that research and teaching for the extension of scientific and technical knowledge would be permitted except where directed toward warlike activities. In addition to such obvious warlike developments as guided missiles, prohibitions included various phases of nuclear research and aeronautics.

The chief prohibition in nuclear research is the mass separation of radioactive substances. The economic condition of Japan is a very effective limitation on research in this direction. In aeronautics, research and organized instruction directed toward the manufacture, design, or operation of any aircraft or devices specifically designed for aircraft are prohibited. This does not prevent the usual studies in meteorology or the normal teaching and research in aerodynamics not specifically applied to aircraft.

The cooperation of the Japanese scientists with the occupation authorities has always been most remarkable. A few scientists have been purged for political or military activities, but none because of a purely scientific activity.

The Japanese have been encouraged in their desire to enter into their normal activities in publication. A group of them have already abstracted about 5,000 of their papers in physics, chemistry, biology, and engineering published during the war, and the abstracts have been forwarded to the appropriate journals abroad. In addition, current research papers are now appearing in foreign journals. The importation of scientific journals is encouraged, but because of monetary exchange difficulties this is somewhat handicapped. Some journals have arrived as gifts, however, and organizations such as the American Institute of Physics and the American Association for the Advancement of Science have given free permission for the translation and reproduction of their publications until other arangements can be made.

There are about 25,000 natural scientists and engineers in Japan today, distributed approximately according to Figure 1. Using the Japanese definition of science, there are approximately 90,000 scientists. An indication of the interest of younger students is given in Figure 2, which shows the approximate distribution among the different fields of science.

SCIENTIFIC LIAISON GROUPS

A very limited technical staff among the occupation authorities soon made it apparent that a better system of liaison was necessary for a broader contact with scientists and their laboratories. In June 1946 a group called the Japanese Association for Scientific Liaison was formed to give at least geographic representation to the authorities in Tokyo. It was the function of this group to gather specific information as required, to help interpret occupation directives to the Japanese, and, by more intimate contact, to bring Japanese problems to the attention of the occupation. Thus, it was designed to be of mutual benefit to the occupation authorities and the Japanese scientists.

This Scientific Liaison Group worked so successfully that the scientists in other fields asked permission to form similar groups. As a result, liaison groups were formed in agriculture, medicine, and engineering. After several months' successful operation of these groups, they asked that they be made more truly representative of Japanese scientists. The groups had attracted the interest of the more active young scientists, who were restricted in expressing their opinions by the older established organizations.

In the meantime, the ineffectivness of the older organizations was freely discussed. The amount of cooperation among the three national bodies was surprisingly small. Considerable duplication, as well as neglect in attending to urgent activities essential to the rehabilitation of Japan, had been noted. The fact that an individual belonged to two, and sometimes three, of these organizations seemed to make little effective contribution toward improving coordination. The active operations of the Liaison Groups and the reticence of the old established bodies soon made it obvious that the roles of the existing bodies would have to be clarified before real progress was possible. The Liaison Groups were asked for advice on the subject.

After the introduction of the question of the desirability of reorganization of the existing national bodies of science to the Liaison Groups, there was a great deal of criticism of the existing bodies expressed by individuals of the Groups. They especially criticized the feudalistic attitude of the Imperial Academy and its influence on the National Research Council. It seemed to be the general consensus that, although most of the members of the Academy had made real contributions in the past, they were now all too old to be capable of real activity and prevented the younger scientists from expressing their opinions. These were only opinions from individuals, however, and the Liaison Groups would give no formal

opinion, since they did not consider themselves a representative enough group to make a formal plan. At that time there was talk of a possible peace treaty, and the Groups probably were afraid of losing the support of the occupation.

During this same period two other developments took place. A bill was successfully engineered through the Japanese Diet for the reorganization of science and the concentration of its direction and control. Because the bill would emphasize the faults of the existing bodies and because this surprise move met so much opposition from the Liaison Groups and the occupation, it was disapproved. Somewhat later, the Imperial Academy approved an occupation plan for abolishing the National Research Council and placing most of its functions under the Japanese Association for the Advancement of Science. This also came as a surprise to everyone, for the Research Council appeared to be the most effective of the three organizations, and the plan had been approved in complete ignorance of most of the members of the Council and the Liaison Groups. The Academy in turn appeared surprised, for it understood from occupation authorities that reorganization plans were to be initiated and formulated by the Japanese themselves. It is not known whether greater understanding was obtained from the reply that the original statement was correctly quoted, but that it was not certain that the Academy truly represented the Japanese scientists.

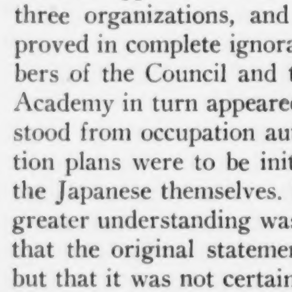
As a result of this activity, the Liaison Groups, the Imperial Academy, the National Research Council, and the Ministry of Education were requested to form a representative body of scientists to formulate plans for the reorganization of the existing scientific bodies. The first step was the formation of Sewanin Kai, a kind of qualifying committee to pass on the qualifications of electors to a planning committee.

In the meantime, the U. S. National Academy of Sciences was invited to send a small group to Japan to advise the occupation authorities on the attitude they should take toward any plans proposed by the Japanese. This advisory group was present in Japan on the inauguration of the "Renewal Committee," a kind of charter or planning group that was to make proposals for new national scientific organizations in Japan. Because of strong criticism from districts outside Tokyo, the new organization was to have geographical representation as well as representation from different scientific fields. Further, the new organization was to reflect a more representative opinion of Japanese scientists, to be adaptable enough to meet the technical problems facing Japan, and to have its charter operate within the new Constitution of Japan.

THE RENEWAL COMMITTEE

The qualifying committee chose the general plan of using an electorate composed of all scientific societies having 500 or more members. Some fifty of these societies existed. There were to be 108 members elected to the Planning Committee, or, as the Japanese termed it, the "Renewal Committee." Each of the seven traditional "faculties" of a full-fledged university—law, economics, literature, engineering, agriculture, science, and medicine—were to be represented by 15 members each. In addition, 3 members were to represent the Society of the Science of History of Japan, the Democratic Scientists' Association, and the Association of Democratic Scientists.

The scientists expressed deep interest and concern in the method and outcome of the election of the Renewal Committee. There were many difficulties in defining eligibility to vote, and shortages of time, paper, and funds for stamps and envelopes to mail ballots. However, it was generally agreed that the selection of the Renewal Committee was



NUMBER OF SCIENTISTS

SCIENCE	ENGINEERING	AGRICULTURE	MEDICINE	TOTAL
3266	9043	2542	10229	25080

Fig. 1. Statistics showing number of scientists and engineers in Japan. (From Ministry of Education Scientific Education Bureau, March 20, 1947.)

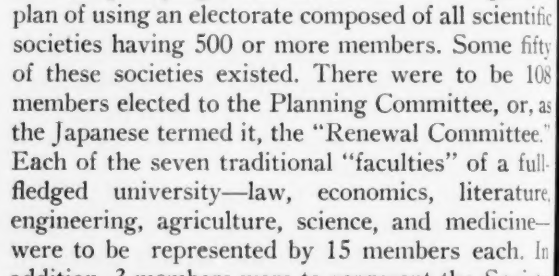


Fig. 2. Number of graduates from universities and institutes of Japan in 1946. *Left,* number of graduates classified by type of institute; *right,* number of graduates classified by faculty.

made in the best way possible under the circumstances. There were some criticisms of the outcome of the election. These were substantially as follows: (*a*) that the preponderance of representation was from Tokyo Imperial University (just over half); (*b*) that the broader Tokyo area had too many members (just over three fourths); and (*c*) that private universities had insufficient representation (less than 10 members).

Despite this outcome, however, the Renewal Committee represented a genuine break with tradition. Of 108 members, only 6 came from the Imperial Academy, only about one half from the large National Research Council (12 of these being recently elected members), and the average age is less than fifty years. It must also be said that preponderance of representation of the Tokyo group rested in some measure on relative quality and not solely upon prestige and influence.

The Renewal Committee keenly felt the criticisms and at one point considered dissolving itself because of them. It was finally concluded that sensitivity to this criticism was a good omen, and the Committee was encouraged to tackle the problem.

The Renewal Committee was inaugurated on August 25, 1947, at the official residence of the Prime Minister in the presence of the Prime Minister, the Japanese Cabinet, the Advisory Group from the United States National Academy, and the heads of the technical sections of General Headquarters of the Supreme Commander for the Allied Powers. The Prime Minister gave assurance that the recommendations of the Renewal Committee would receive a sympathetic hearing by the Japanese government. The Advisory Group expressed confidence in the Renewal Committee, indicated the importance of its task, and signified that it considered the Renewal Committee the most representative that could be had under the circumstances. The members of General MacArthur's Headquarters gave assurance that the problem of reorganization of the national bodies of science was the problem of the Renewal Committee, and that the main interest of SCAP would be to see that recommended organizations were composed of a representative group of scientists, capable of attacking the pressing technical problems facing Japan, and in so doing to preserve sound governmental procedure under the new Constitution in Japan.

Immediately after its inauguration, the Renewal Committee began accepting plans for consideration from the Science, Engineering, Agriculture, and Medical Liaison Groups, the Imperial Academy, the National Research Council, private universities, and union groups. The Committee held eight formal meetings in addition to numerous meetings of subcommittees. During these meetings the proposals made by all interested groups were considered. The meetings were not attended by occupation personnel except on specific invitation to discuss a specific point.

At the conclusion of the eighth general meeting, a draft of a bill for a new Science Council was prepared and suggestions for a technical advisory group to the cabinet, along with a report of the activities of the Renewal Committee, were presented to the Prime Minister.

The recommendation of the Renewal Committee for a new Science Council was passed by the Diet on July 5, 1948. The proposal for a governmental body called the Scientific and Technical Administration Commission was withheld for further consideration pending passage of the Science Council Bill.

THE SCIENCE COUNCIL OF JAPAN

The present National Research Council is to be dissolved, the Japanese Society for the Promotion of Science is to become solely a private and independent body that can receive money from the government only for possible specific contracts. The Imperial Academy of Science is to become solely an honorary body whose membership shall be determined by the new Council.

The Science Council will have 210 members, composed of 30 each from the seven traditional "sciences" of Japan. The members are to be chosen by free election by vote of the scientists of Japan.

The qualifications for the voters appear quite liberal. Graduation from a university or college with three years' postgraduate experience, or a letter of recommendation from one of the professional societies or from a reputable research institution are examples of qualifications required. The Renewal Committee estimates that with the criteria they have chosen, there will be approximately 90,000 eligible voters. There is still some objection to the method of election, from groups with special interests. Their chief complaint appears to be that their organizations are not properly represented. The Renewal Committee, however, insists that scientists who are members of these special groups will receive appropriate representation through the vote of the individual scientists.

For the election, to be held every three years,

Japan is divided into seven districts, each district to have representatives in the seven fields of science; this will give 49 members of the Council who will have been elected on a regional basis in order that geographic representation be obtained. The remaining 161 members are to be members at large elected by a nation-wide vote. The purpose of the latter is that the outstanding men of science can be elected and that they will not be handicapped because they live in a dense population of scientists.

The functions of the Science Council of Japan are given in the *Preamble* and Chapters I and II of law No. 121 of 1948, which follow:

The Science Council of Japan shall hereby be established, on the conviction that science provides the basis of a cultural country and with a view to fulfilling its mission of contributing by the joint will of the scientists throughout Japan to the peaceful rehabilitation of this country and promotion of the welfare of human society as well as to the advancement of science of the world, in cooperative relations with academic societies of foreign countries.

Chapter I

Establishment and Aim

Article 1. The Science Council of Japan shall be established by this Law which shall be called the Science Council of Japan Law.

All transactions of the Science Council of Japan with the Government shall be through the Prime Minister.

Expenditures of the Science Council of Japan shall be defrayed from the National Treasury.

Article 2. The Science Council of Japan shall aim at promoting the development of science and permeate it into administration, industry and the nation's life as the representative organ, internal and international, of the scientists of this country.

Chapter II

Functions and Powers

Article 3. The Science Council of Japan shall perform the following independently:

a. To discuss important matters concerning science, and to make exertion for the realization thereof.

b. To coordinate scientific researches for the enhancement of their efficiency.

Article 4. The Government may seek the opinions of the Science Council of Japan on the following:

a. Compilation of budget for and distribution of the government grants and subsidies in order to help scientific researches and experiments or to promote science in general.

b. The policies on the compilation of budgets concerning the expenditures of the institutes, laboratories under the jurisdiction of Government, and of commissioned researches.

c. Important measures particularly requiring deliberations by expert scientists.

d. Other matters recognized as proper to be referred to the Science Council of Japan for deliberation.

Article 5. The Science Council of Japan shall place itself available to the Government for recommendations on the following:

a. The schemes for promotion of science and advancement of technology.

b. Measures for the utilization of the results of scientific researches.

c. The schemes concerning the training of scientific researchers.

d. Reflection of science on administration.

e. Permeation of science into national life and industry.

f. Other matters necessary for the fulfillment of the aims of the Science Council of Japan.

Article 6. The Government on request from the Science Council of Japan may submit data or explanations or set forth their views.

There are seven chapters in the law, but the first two are sufficient for present purposes. The Renewal Committee would prefer that the Science Council be financially independent of the government, but under the present conditions in Japan this is impossible.

THE SCIENTIFIC AND TECHNICAL ADMINISTRATION COMMISSION

Although the Science Council is to be democratically elected and is to represent all fields of scientific activity and all regions of Japan, it is nonetheless elected by a restricted electorate and therefore cannot impose its will on the government. Its control must come from its prestige and intelligent advice.

In order that the government can appropriately consider and implement the proposals of the Science Council, the Japanese Renewal Committee has recommended to the government that a Scientific and Technical Commission be set up in the Prime Minister's office.

The Commission is to be appointed by, and be under the jurisdiction of, the Prime Minister. The maximum number of members is to be twenty-four, at least half of whom shall be persons of scholarly attainment and experience. In the appointment of the latter class the recommendations of the Science Council shall be respected.

The functions of the Commission are to deliberate and make recommendations on the following matters:

a. Measures necessary for the administration of the reports or recommendations made by the Science Council of Japan.

b. Selection of matters which are to be referred to the Science Council of Japan for consultation by the government.

c. Method of execution of international enterprise concerning science and technology which should be carried out by the government.

d. Liaison and coordination of matters relative to science and technology that are under the jurisdiction of the government administrative agencies.

A proposed bill for the establishment of the Commission is now (November 1948) under consideration by the Japanese Diet.

The future holds great responsibility for Japanese scientists and engineers. With a large and increasing population density, limited resources, and higher labor costs than those of the old feudalistic system, the Japanese technologists hope they have a system, at least, which will allow them to assume their new responsibilities in a flexible and effective manner.

This survey has been an attempt to show at least the outward changes in the scientific structure during the occupation of Japan. It probably would be of greater interest—and difficulty—to describe the changes made in United States scientists who, because of the war, have a particular interest in Japan and Japanese scientists. Actions such as that of the United States National Academy in sending two groups of its most eminent scientists to advise the Supreme Commander for the Allied Powers on scientific policies; permission for translation and reproduction of articles from the journals of such organizations as the American Association for the Advancement of Science and the American Institute of Physics; and the invitation to Japanese scholars from such institutions as the Institute of Advanced Study, demonstrate that the scientists of the United States recognize their new international responsibilities.

日本科學

Purchase of Foreign Books and Periodicals

Books and periodicals published in "hard" currency countries like the United States may now be purchased by persons living in "soft" currency countries through the medium of UNESCO book coupons. During the 1948–49 experimental period, the coupons will be available only in China, Czechoslovakia, France, India, Poland, and the United Kingdom and primarily to meet the needs of educational, scientific, and cultural institutions. There will be limited free distribution in Austria, China, Czechoslovaka, Greece, Hungary, Italy, the Philippines, and Poland.

The coupons and an explanatory leaflet are now available from the National Distributing Body for UNESCO Book Coupons in each country. The book, pamphlet, or periodical desired may be bought through a bookseller or directly from the publisher, but the exact price and other details should be determined in advance. The coupons come in denominations of 25 cents to $10.

AMERICA'S MYTHICAL SNAKES

CLIFFORD B. MOORE

Mr. Moore has been director of the Trailside Museum, Springfield, Massachusetts, for the past eleven years. He is the author of Book of Wild Pets *(Putnam's, 1937) and of a forthcoming source book to be entitled "Myths and Superstitions in Zoology."*

FOR choice items of folklore in great number and variety, the subject of snakes has everything to offer. Ever since the curse was officially placed on serpents in the Garden of Eden, their reputation has suffered, and doubtful stories about them have increased to such an extent that a very large book indeed would be required to record all the strange habits and remarkable abilities they are purported to possess, together with the inevitable eyewitness testimony.

Probably foremost in the gallery of America's mythical snakes is the serpent that is supposed to take its tail in its mouth and roll about like a hoop. This unique creature is said to have a poisonous sting in its tail, which is launched at its enemy from a rolling position.

There appears to be no classical or European analogue of the American hoop-snake story. Our subject is first encountered in colonial days, and one naturalist of the period described a "water viper" he encountered as having a tail ending in a blunt, horny point about half an inch long.

> This harmless little Thing [he says] hath given a dreadful Character to its Owner, attributing to him another Instrument of Destruction besides that he had before, imposing a belief on the Credulous, that he is the terrible Horn-Snake. armed with Death at both Ends, tho' in reality of equal Truth with that of the two-headed Amphisboena; yet we are told, that this fatal Horn by a Jerk of the Tail, not only mortally wounds Men and other Animals, but if by Chance struck into a young Tree whose Bark is more easily penetrable than in an old one, the Tree instantly withers, turns black and dies.

In North Carolina the horn snake that could conveniently roll like a hoop was said to hiss like a goose and to kill its victims with its horny tail. Certain professed witnesses reported "that a small Locust-Tree, about the Thickness of a Man's Arm, being struck by one of these Snakes, at Ten a clock in the Morning, then verdant and flourishing, at four in the Afternoon was dead, and the Leaves red and wither'd." In Pennsylvania the death of a tree from horn-snake venom required twenty-four hours, in New Jersey two days. It was observed in Virginia that the horn snake upon striking its tail into a musket butt could not disengage itself.

Not all the good hoop-snake stories come from colonial sources. The following somewhat modern tall story, not meant to be taken literally, as the colonial stories are, is based upon the fundamental premise that the hoop snake really exists. I have heard it repeated with minor variations or with added color, depending on the section of the country and the imagination of the individual narrator.

> A youth who lived in Oklahoma had a sweetheart on a ranch several hundred miles to the south, on the Rio Grande. Young women are few and far between in the great open spaces of the Southwest—at least pretty girls like Nell. Our hero, who rode a bicycle and whose name was Tom, had a rich rival who sped over the wastelands on a motorcycle.
>
> One day Tom was pedaling in the scrub country when his rival sped by on his motorcycle. Before the latter disappeared in a cloud of dust, he tauntingly sang out that he was headed for Nell's to propose to her that very evening. Our hero turned his bike about forthwith and headed for the Rio Grande too—with a wild hope that he might beat his rival (who could, of course, run out of gas many miles from nowhere) and so be the first to propose to Nell. An hour of hard pumping on the bicycle at top speed of fifteen miles per hour convinced Tom he would never reach the Rio Grande before dark and that Nell, in all probability, would be lost. As these thoughts gloomily crowded through his mind, there was a sharp report and a hiss of air. A cactus spine had penetrated the front tire of the bike!
>
> Now thoroughly disconsolate, Tom abandoned his two-wheeled conveyance and sought the shade of a near-by yucca. As he conjured up dismal images of Nell and his rival, he observed a pair of hoop snakes rolling about and playing tag with each other in the brush. Inspired, Tom ripped the rubber tires from his bicycle, captured the hoop snakes, mounted them on the wheels, and again pedaled south—to Nell and paradise!
>
> Being a bit thicker than the original tires, the hoop snakes rubbed against the frame of the bike as the wheels spun around. Quite naturally this irritated and excited the energetic serpents, so they increased the speed of Tom's bicycle to 40 miles per hour. Suddenly running over the sharp spines of a horned toad (*Phrynosoma cornutum*, of course), half buried in the sand, the hoop snakes were really shocked into action and in a wild and glorious burst of speed sent Tom and his bike forward at the un-

heard-of speed of 250 miles per hour. Soon our hero observed his rival's motorcycle some distance ahead and, in a fraction of a minute, passed it in a cloud of dust. In another half an hour the Rio Grande and Nell's ranch came into view.

It was by now quite impossible to control the speed of the infuriated hoopsters, and Tom saw he would have to resort to the spectacular if he didn't want to visit the interior of Mexico. With great presence of mind he steered directly under a grape arbor beside Nell's front door and managed to reach up and catch the main trunk of the vine in his hands. The snake-propelled vehicle continued southward and in no time at all was in Mexico City. It took Tom a good five minutes to stop whirling round and round the grapevine stem, so great was the speed at which he had caught it. On regaining his equilibrium he promptly proposed to Nell, and, as in all true romances, the two lovers lived happily ever after.

Another hoop-snake story (which might well prove the end of all hoop-snake stories) comes from Karl P. Schmidt:

A legendary boyhood friend of the distinguished Chicago zoologist liked nothing better than rambling through the woods, and on one of these frequent strolls his attention was drawn to a stately pine whose needles were becoming brown and withered in death. Having observed this same tree in the full vigor of life only a few days before, he was naturally curious to learn the reason for its sudden demise. Upon reaching the tree he was astounded to find a large hoop snake with its poisonous caudal spine so firmly embedded in the trunk that the serpent could not extricate itself.

His youthful heart filled with pity at the unfortunate hoopster's predicament, and to prevent it from slowly starving, our hero liberated the snake and went his way. The hoop snake was so grateful for this kindness that it followed its benefactor wherever he went from that time on. Naturally, a busy farm lad could not devote much time to capturing rodents and other animal food for his pet and constant companion: he therefore trained it to become a vegetarian, with mashed potatoes as its principal food.

This happy association of snake and boy persisted for some months, but, alas, an unhappy event occurred! One day as the snake was rolling rapidly downhill behind its young master, tail in mouth, as is customary with all hoop snakes, the serpent struck a stone, and the shock of the collision was such that its caudal spine scratched the roof of its mouth, causing it to bleed. Going completely mad at this sudden taste of its own blood, the hoop snake reverted to its carnivorous diet and devoured itself tail-first, before the horrified boy's very eyes.

When we speak of the hoop snake, horn snake, or stinging snake, a single species, *Farancia abacura*, is implied. The circular position of the snake lying prone and engulfing its prey suggests a "hoop;" the tail spine suggests a "horn;" the sharp end of the spine prodding the human hand suggests a deadly "stinger." But here are the essential facts. No snake known to science is capable of rolling like a hoop or of hurling itself bodily against a tree. Besides, no snake is possessed of a caudal stinger. Certain insects and all scorpions have tail stingers, but snakes do not. The business and offensive end of a snake is always its head and never its tail.

Farancia is a large, brilliantly colored, harmless serpent of the South. Because this snake prefers swamps, muddy areas, and the edges of ponds, it has been given the common name of "mud snake." The hard, hornlike tail spine provided by nature is believed by the layman to be a "stinger," and in certain parts of the South the stinging snake is more feared than the rattlesnake or the cottonmouth moccasin.

Various suggestions have been offered to help explain the function of the tail spine, but none of these has been based upon extended observation. Since many specimens prod the hand with the tail spine (young specimens with sharp spines sometimes draw blood), the idea has developed that the snake is simply protecting itself. Another suggestion is that the tail spine is driven into the ground when the snake is struggling with *Amphiuma* or dragging this amphibian prey out of a hole. One writer states that "it probably functions during burrowing." George P. Meade, a leading authority on *Farancia,* has witnessed no such actions as the foregoing, although he has kept and studied the snake in question over long periods of time.

It is always a source of surprise to those who examine a mature horn snake to discover that the famed horn, or sting, is actually no sharper than a blunt pencil point although, of course, that of smaller individuals is generally sharp. This difference appears to be significant, since Meade and others have repeatedly observed that the younger individuals utilize the sharp spine as a

goad when the amphibian prey bites and holds onto the snake. Under these circumstances, particularly when seized near the head, the snake stabs the victim so sharply with the spine as to cause it to release its hold. Blood is frequently drawn, and long, deep scratches are inflicted on the soft body of the amphibian.

It is generally agreed among herpetologists that the hoop-snake myth is almost invariably identified with the mud snake, and it is likewise felt that the possession of the tail spine is in some way related to the hoop snake. A possible explanation in the mind of Ditmars is the "habit of *Farancia* of occasionally lying in a loose coil . . . almost forming a circle" and having the appearance of "a discarded bicycle tire." But, as Meade points out, a much more definite basis for the hoop-snake story may be seen while *Farancia* feeds upon *Amphiuma*. The larger and mature snakes have

a tendency to rotate on their longitudinal axis as they grasp either head or tail of their victim and start to swallow it.

Wherever members of the widely distributed whip snake (*Coluber*) group are to be found, there is likewise to be found the story that these serpents are addicted to the infamous practice of wrapping themselves about people and thrashing them. In certain versions of the story the human victim is said to have been whipped to death!

Whipping or flagellation by snakes is a physical impossibility, and, furthermore, whip snakes are not constrictors, and they cannot hold people prisoners. It is true that the form and coloration of the scales on the tail of the snake, particularly the Eastern coachwhip, do suggest the appearance of a braided whip, from which it derives its common name. But the real basis for the whip-snake belief lies without doubt in the serpent's defensive behavior when confronted by a member of the human race. Thus, when cornered and molested, when prodded with a long stick, this snake with a characteristic display of bravado elevates its tail to an upright position and nervously vibrates it like its close relative, the blacksnake racer. Captive specimens, when held by the neck, almost invariably shake the body violently. The illusion is then complete.

Lawson, in his work *A New Voyage to Carolina* (London: 1709), describes a whip snake thrashing a rattlesnake. The only element of truth in the account is the fact that whip snakes do occasionally attack and devour rattlesnakes: but there is no thrashing or whipping in the process.

The whip snakes, in common with the racers, are members of what is probably the fastest-moving genus of snakes in existence. As told in the legend that has developed around the whip-snake group, a person must run extraordinarily fast and according to Olympic standards if he is to escape being caught and whipped. Why the whip snake desires to torment human beings, and what happens to them upon death, has never been made clear in the mythology of the subject. Certain it is that adult-sized whip snakes measuring 3.5–4 feet in length and no larger in circumference than a woman's wrist, could not devour a human being, and no authentic instances have been recorded of alleged lethal attacks.

Some of the stories about the whip-snake draw largely for their color upon the details of the chase, of assuming that the serpent has a disposition to relentlessly pursue the object of his quarry. A. C. Stimson claims, in an article that appeared in the *Antivenin Bulletin,* to have been followed by a whip snake.

> It is true [he writes] that a Coach Whip will, on rare occasions, follow an unaccustomed sight—for instance, I was followed for probably a quarter of a mile by an unusually large snake of this species. This happened in a small prairie that was surrounded for a radius of about a mile with a semi-tropical thicket. I noticed the snake just as I left the foliage. With his head, which angled about ninety degrees from his neck, reared about two feet from the ground he was calmly watching my every motion. When our respective curiosity was satisfied, I continued my tramp. After a few hundred feet I paused for some trivial reason and was surprised to see what I then thought was another snake in the same identical posture as the other one which I had just left. While it is nothing very uncommon to run across a Coach Whip on the Texas prairie, I had never before seen two such large ones in a short a distance. This time I took a few steps in its direction, and with the speed of a rabbit and the smoothness of running water it poured itself into a scrubby bunch of myrtle, but only when I had approached to within a few feet.
>
> I then walked toward my destination, but watched back for the snake to again erect itself. Imagine my surprise to see, instead, the grasses (about a foot high) being disturbed with that waving motion that only a snake in rapid transit can make, and that disturbance headed directly toward me. I then knew that I was being chased, by the terrible lash which, according to tradition, would soon overtake me, wrap itself about my body and thrash me with its tail until I died in terrible agony; and that, still upholding tradition, I should try my utmost to reach the nearest tree for salvation. . . .

The popular notion that snakes can outdistance human beings on open ground (which would include prairies) has been disproved by herpetologists. In controlled trials, Dr. Walter Mosauer found the fastest speed by any of his subjects to be 3.6 miles per hour. The speediest subject was, of course, a member of the *Coluber* group, which includes the so-called racers.

Certain snakes of the Old World have long been credited with guiding other snakes away from danger, and it is reasonable to assume that early settlers in America were quick to transfer this imaginary ability to our native snakes. At any rate, the pilot blacksnake (*Elaphe obsoleta obsoleta*) is our most famous snake pilot, and, since it inhabits the same rocky hillsides and ledges as the banded rattlesnake and copperhead, it has been given the distinction of piloting these venomous serpents away from danger—hence its common name. The pilot backsnake, also called the mountain blacksnake, is frequently confused with the common blacksnake (*Coluber constrictor constrictor*), from which it differs in possessing keeled scales and a highly polished appearance; in the true blacksnake the scales are smooth and have a satiny luster. The

pilot blacksnake, moreover, is a powerful constrictor, whereas the common blacksnake, despite its scientific name, has no constricting ability. Among the venomous snakes, the copperhead is popularly supposed to have guiding or piloting abilities and is said to serve the rattlesnake in such a capacity. Thus, the belief goes, whenever one sees a copperhead, a rattlesnake may appear on its trail.

Some of the most bizarre and curious snake stories center around those creatures of retiring and secretive habits. This is only natural. In the case of the "glass," or "joint," snake we have, in the popular mind, a serpent which, like no other animal, has the ability to break up its body into small pieces, reassemble itself at its convenience, and resume a normal existence.

From a colonial traveler and writer we learn that the brimstone snakes of North Carolina, being brittle as glass, were easily broken; but, according to a North Carolina doctor, "several in these parts confidently affirm, that if they remain in the same place untouch'd, they will joyn together again."

The traditional American glass snake is not, however, a true snake but rather the legless lizard *Ophisaurus ventralis.* This insect-eating lizard of the family Anguidae, no longer possesses true functioning legs since, owing to its burrowing activities in loose soil and under decaying tree bark, it has no further use for them. It is a fact that the legs of numerous burrowing lizards have degenerated to the point where they are merely useless flaps of skin lying along the body, completely incapable of aiding the creature in locomotion—or they have disappeared altogether. The glass snake, or lizard, does bear certain superficial resemblances to a true snake, but upon close examination it can be readily distinguished from a snake by the presence of well-developed eyelids and ear openings. In lieu of the broad crawling scutes of a snake, the abdomen is provided with many rows of smooth and overlapping scales, which are of no practical value in locomotion. The upper surface of the body is of a glassy smoothness, as one of the lizard's common names suggests.

When pursued by an enemy such as a king snake or some mammal, the lizard attempts to slip away. But, since its enemy can travel much faster, the lizard would be in great danger of being captured and killed save for an apparently wise provision of nature. Thus, when the pursuing king snake overtakes its prey, the lizard can, with a sudden twist, cause its tail to snap completely off at a special breakage plane. Sometimes the fragile appendage will, upon a light blow or two, shatter into several pieces. Fortunately for the lizard, the muscle bundles near the base of its tail are so arranged that they expand and close the arteries, preventing loss of blood. The tail or sections of the tail have a very active reflex motion, which causes them to twist and wriggle with great energy for a short time after breakage, and these are much more conspicuous than the lizard's body. The king snake is thus preoccupied with the wriggling tail in its mouth—so much so that the lizard makes an inconspicuous and successful escape. The king snake, upon releasing the tail from its mouth momentarily, in order to secure a more convenient head-first position of its victim for swallowing, suddenly discovers that this "victim" has no head and vital organs. Searching about for the head and remainder of the body, it can find neither.

The lizard will very soon grow a new, though perhaps shorter, tail, but it is not true that pieces of the cast-off caudal appendage will join together again. The new tail never has the same shape or color as the original one, nor are the scales as even as in the old one. Of all its body structures or organs, the glass lizard can regenerate only its tail, which comprises about two thirds of its body. Consequently, if the shorter but vital part of the body is severed, all life will cease. The loss of a tail is rarely fatal to any reptile, and many other kinds of lizards besides the glass species have the same ability of regenerating the lost part. Other kinds of animals, including the crayfish and starfish, can part with appendages or less vital organs and grow replacements.

It seems to be common knowledge that the thief among snakes is the milk snake. Two concomitant elements enter into the main fabric of the story about this much-slandered creature, and both of these arise from erroneous inferences that have been drawn to explain the simple behavior of a most useful and misunderstood serpent. In the first place, there is the fact that cows on occasion give a decreased milk output, or they may go entirely dry. Second, certain snakes of the species *Lampropeltis triangulum* are sometimes observed frequenting barns or pastures in close proximity to cows. Moreover, the snakes have been seen on certain rare occasions drinking milk from a saucer which had been set out for the cat. The farmer now enters the picture: not being able to assign a good reason for his cows going dry, he blames the snake.

People generally react in one of two ways when

they meet a snake. If it is reasonably small and seeks escape, they may try to kill it with a convenient club or stone. If it is large, they flee its presence. The milk snake, being a reasonably small serpent, is often killed when it unfortunately gets in the way of mankind, and, like so many snakes that fall the victim of club and stone, its body is somewhat mutilated in the process. Were such a mutilated snake a gravid individual, the crushed eggs, as one herpetologist suggests, would give forth a milklike fluid, and this might be construed as confirmatory evidence for the belief in the snake's peculiar ability to milk cows.

There are, however, some very serious objections to the idea that *Lampropeltis* milks cows. The "eyewitness" accounts of snakes in the act of milking almost invariably have an evidential defect (common in a number of snake stories)—they relate an event long past. One distinguished herpetologist, Clifford H. Pope, has said in his work *Snakes Alive* that

> when a farmer insists that his sixteen-quart cow has fallen off a quart a day because of the theft of her milk by a 30-inch snake, he is accusing the snake of consuming about eight times its own volume of food every twenty-four hours! This is easily proved by measuring the amount of liquid displaced by a 30-inch milk snake, a very simple experiment. . . . The volume of a large 37-inch snake of this species it only half a pint. Snakes are well known to eat big meals but not that big! Here are a few other reasons why a milk snake could not perform such a feat:
>
> Unless it carried its own milking stool, it could not reach the cow's udder. If wrapped tightly around the cow's leg near her udder the snake would obviously be unable to force much milk into its body.
>
> There are no sucking muscles in its throat that would enable it to get the milk out, for milking a cow is no simple task, as every milker knows. Nor are there any valves or sphincters in its throat to keep the milk in under pressure of the distended stomach, body-wall, and skin.
>
> The six rows of sharp teeth in its mouth would not tend to soothe the cow; on the contrary the pain would drive her frantic.

The presence of the milk snake in the vicinity of barns, farmyards, and pasture lands is due for the most part to its being attracted to such places by mice and young rats, which are its natural food. One biologist, H. A. Surface, analyzed the stomach contents of 42 specimens of *Lampropeltis* from Pennsylvania and found that field mice comprised 72 percent of the food. As every farmer realizes, it is field mice that raise havoc with the grain supply.

The milk-snake story, variously modified in many parts of the world, is nowhere so grotesque as in Brazil. There it is believed that the snake secures its milk supply, not from cattle, but from human beings. According to this legend, the snake which frequents the huts of the Indians in the night, interrupts the feeding of the baby while the mother sleeps, inserting its tail into the child's mouth in order to soothe it.

Certain writers in the field of natural science, in denying the truth of the milk-sucking habit of milk snakes, have stated categorically that snakes do not drink milk, and they have cited instances of the snakes refusing to take such liquid nourishment. This is not entirely true. Many snakes do drink milk if it is provided for them, and, moreover, it is a common practice in India for milk to be placed in saucers in some zoological parks in place of water, this to ensure the snakes' obtaining nourishment when they refuse other food.

Another mythical serpent, confined to the rural scene, is the horsehair snake. In its adult stage the worm *Paragordius varius* is long, extremely slender, and unsegmented (in distinction to the earthworm). Its occasional appearance in animal drinking troughs, especially those of horses, and its vague resemblance in form to a horse hair that may float on the same drinking water, have given it a misleading appellation, "horsehair snake." However, in the animal kingdom, this worm is many phyla below, and it is far removed from the phylum Chordata, to which snakes belong.

Paragordius deposits its eggs in long strings in any convenient shallow pond, pool, or receptacle of water. The larvae penetrate the bodies of young mayflies and other kinds of aquatic insects; they later escape from their hosts and seek out second hosts, generally beetles, crickets, or grasshoppers. In the body cavity of the second host the larvae continue their development, eventually emerging and seeking water, in which they become sexually mature worms.

It would appear to be an easy and logical step in the minds of many people unfamiliar with the animal kingdom and natural biological processes to assume that when a hair is swallowed, a live worm or snake develops from it inside the stomach or intestines. Tapeworms and other visible and smaller human parasites are responsible for this old and venerable item of zoological folklore. Thus, by association, a hair which was once a part of a live animal becomes alive again, only in a different form.

Some people believe that one may swallow a young snake while drinking spring, well, or even tap water. Accordingly, the ingested serpent is said to live and to develop into an adult specimen of its kind inside the human stomach or intestines.

This notion undoubtedly gained new adherents a few years ago in the Borough of the Bronx, New York, when numerous varieties of plankton and other fresh-water organisms were observed in the unfiltererd drinking water following the opening of a new source of water supply from upstate New York.

The late Clifton Johnson, in his authentic collection, *What They Say in New England*, reported the story

that once there was a certain child that took large quantities of food, in particular a great deal of milk, yet became more and more emaciated. One night when the child was sitting at the table with a bowl of milk before it, of which it had not eaten, a great snake put its head out of the child's mouth. Apparently it was hungry, had scented milk, and came up out of the child's mouth to get it. The child's father was by, and he grabbed the snake by the neck, and pulled it out. It was four feet long.

Dr. Morris Fishbein, editor of the *Journal of the American Medical Association*, has pointed out that because someone once saw an X-ray picture of a stomach tube in the human stomach, the idea grew that it would be possible for a person to swallow a snake egg and to have it develop in the stomach. This illuminating detail brought out by Dr. Fishbein might well be termed a recrudescence of our hair-to-snake myth but with machine-age applications.

A very curious snake, whose existence is commonly brought to the attention of zoo and natural-history museum curators, is the Northern water moccasin. If the uninformed person happens to be swimming in the waters of Lake Champlain, New York, Sebago Lake, Maine, or any other Northern body of water for that matter, and if he inadvertently steps upon a crayfish or bullhead and receives a response therefrom, or if he sees an eel or a banded water snake swimming in its natural habitat nearby—then such a creature must be the poisonous and ill-reputed water moccasin. It is entirely possible too that leeches, which elongate their bodies while swimming, as well as numerous kinds of harmless snakes that occasionally swim across bodies of water, are confused with water moccasins.

The actual range of the venomous water moccasin includes the Dismal Swamp, Virginia, to Florida and the Gulf States, and Arkansas to Illinois. The dreaded water moccasin of our Northern states all too often turns out to be the banded water snake (*Natrix sipedon*), erroneously called the "water rattle" and "water pilot" in certain localities. This nonvenomous serpent frequents the shores of ponds and streams, is a good swimmer, and feeds upon frogs and sluggish fish. It does not molest human beings unless molested by them.

All green snakes are held to be poisonous. They are supposed to be filled with a green venom, which the green color of their bodies suggests. But the fact is this: venomous green snakes do make their home in the tropics but there are none in North America. It can be said that such a superficial distinction as color is never an indication of a snake's venomous character any more than is the shape of the snake's body or head. The real reason a green snake is green is probably that nature intended this animal to blend in with its grassy surroundings and so escape detection by certain of its natural enemies or its potential prey.

Our smooth green, or grass, snake (*Opheodrys vernalis*) is remarkably gentle and inoffensive, and it is almost impossible to induce it to bite when handled. One professional snake-swallower of my acquaintance prefers snakes of the green and ring-neck species in performing his act since, as he maintains, these are not "nasty" like the garter snake and other kinds.

Inasmuch as our green snakes are probably the most characteristic grass-inhabiting serpents, they are eligible candidates for the term, in its literal sense, of "snake in the grass." But, unfortunately for these inoffensive little serpents, and very unjustly too, "snake in the grass," as applied to certain members of the human race, has come to mean that these people have a reputation for underhanded and surreptitious acts.

No gallery of American mythical snakes would be complete without mention of the celebrated blow snake, sometimes known as the "hissing snake," "blowing viper," "spreading adder,"or "puff adder." This is none other than the common hog-nosed snake (*Heterodon contortrix*), whose antics, in times of danger, have given it a notoriously bad reputation. Numerous stories are to be heard of the way in which this harmless serpent blows its poisonous breath or spits venom from a distance into the innocent observer's face, thereby producing burns, blindness, infection, or convulsions. Even though the majority of these stories are related secondhand, there seems to be no dearth of actual eyewitness "evidence" of such personal encounters, with consequent poisonings.

Since the hog-nosed snake is fairly common in sandy locations, where it preys upon toads, it is not a difficult matter to check on its bad reputation

and to obtain actual observations on its behavior. When first approached, the hog-nosed snake flattens its head and neck, inflates its lung, and then exhales, hissing loudly. To make its display of ferocity even more effective, the snake elevates the forward third of its body from the ground. All this tends to give it a formidable and threatening appearance, even more so than that of any Indian cobra of corresponding size. If the intruder does not quickly retreat from the presence of the bluffing serpent, it may pretend to strike, with continued hissing, but with closed mouth. In fact, one cannot induce this snake to bite at any time. Provided the intruder is intimidated and departs the scene, the snake quickly deflates and either seeks a place of security or resumes its normal activities.

The stories about the poisonous breath of the hog-nosed snake are pure fiction, for its breath has no effect on eyes, nose, or skin. If the snake is picked up with a stick or held in the hand, a pungent and disagreeable secretion may be emitted from the anal scent glands, and, to an already frightened or doubtful person, this odor may be strongly suggestive of a poisonous breath.

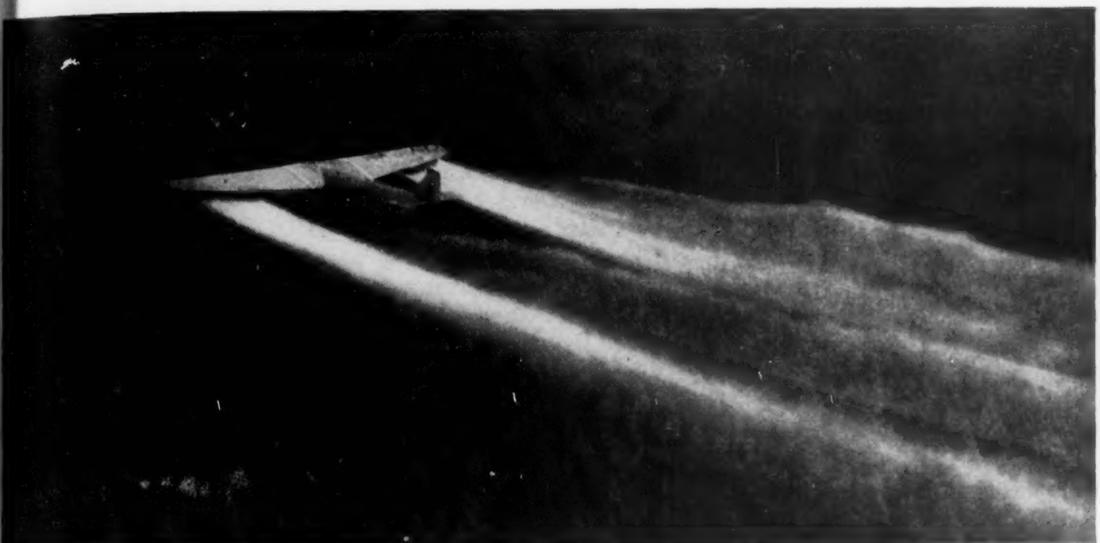
If a snake of this species is further molested, and especially if it is abused or injured, or if its first hissing and puffing antics fail to intimidate, it resorts to a second bluff, that of the death feint, or playing possum. Here the snake writhes and squirms convulsively with mouth wide open and as if in mortal agony. Then it rolls over on its back, the head is rubbed in the dust, there are a few convulsive movements, and finally all movement ceases. To all outward appearances the snake is dead, and it may be moved about or carried on a stick while remaining inert and playing dead. If placed on the ground rightside-up, it will promptly roll over again on its back, thus giving itself away. According to this creature's instinctive behavior pattern, it appears that in order to seem thoroughly dead a snake should be lying on its back. When danger appears past, the snake twists and raises its head slightly; if the coast seems clear, our bluffer turns over and crawls away.

Included in the fanciful category of snakes that hiss, blow, or spit poison, are the ones mentioned by Father Charlevoix, the eighteenth-century Jesuit missionary and traveler. Near Detroit, he related there existed two rattlesnake islands in whose vicinity the very air was infected. Another colonial traveler learned that certain islands at the west end of Lake Erie were overrun with a species of small, speckled snake about 18 inches long which hissed poison into the atmosphere:

> When any thing approaches, it flattens itself in a moment; and its spots, which are of various dyes, become visibly brighter through rage: at the same time it blows from its mouth with great force a subtile wind, that is reported to be a nauseous smell; and if drawn in with the breath of the unwary traveller, will infallibly bring on a decline, that in a few months must prove mortal, there being no remedy yet discovered that can counteract its baneful influence.

Real blow snakes do exist, but not in America. These include several common cobras and cobra allies. The specialization in question is undoubtedly for the purpose of repelling antelopes and equally dangerous, though unwitting, foes. The veldt-ranging cobras of Africa learned to expel their venom in a fine spray for considerable distances and with a fairly good aim at the eye. The poison is not caustic and the skin remains unaffected; prey cannot be secured by this means, but the moist eye allows so rapid an absorption that sharp pain and subsequent brief photophobia immediately result from the venom contact.

SCIENCE ON THE MARCH



NEW ORGANIC INSECTICIDES

DURING the last five years a bewildering array of new insecticides has appeared on the market. The prospective user of these products is confused by conflicting statements made by their promoters. Each product is claimed to be more potent than DDT as an insect killer, and at the same time it is heralded as harmless to man. What are the merits of these new materials?

The new insecticides that have attained commercial recognition are the chlorinated compounds DDT and its methoxy analog, TDE [1,1-dichloro-2,2-bis(*p*-chlorophenyl)ethane]; benzene hexachloride; chlordane; chlorinated camphene; 1,1-bis(*p*-chlorophenyl)ethanol; bis(*p*-chlorophenoxy)methane; and the phosphorus compounds hexaethyl tetraphosphate, tetraethyl pyrophosphate, and parathion.

Of the chlorinated organic insecticides DDT is the best known; everyone has heard of it, in fact. During the war the armed services used millions of pounds of DDT in controlling the body louse, flies, mosquitoes, bedbugs, and cockroaches. At the close of the war DDT was made available to civilians, and large quantities of it have been used for the control of the codling moth, Japanese beetle, gypsy moth, pink bollworm, white-fringed beetle, potato flea beetle, and numerous other insects.

Trimotored Ford airplane sprays DDT in Idaho mountain forests for the control of the Douglas fir tussock moth. Airplanes now carry huge loads of liquid insecticides. Sprayed over great acreages in minute amounts, these control insects in extensive or otherwise inaccessible areas. (USDA photograph.)

DDT, $C_{14}H_9Cl_5$, was first synthesized by a German student in 1874, but no use was found for it until 1939, when a Swiss chemist, seeking a substitute for lead arsenate, found it to be poisonous to the Colorado potato beetle. It is made from chloral (the active ingredient of knockout drops) and chlorobenzene (a coal-tar product) and contains 50 percent of chlorine. In 1947 the production of DDT in the United States amounted to 50,000,000 pounds. Originally it sold for $1.60 a pound, but the wholesale price recently tumbled to 29 cents a pound.

DDT is a white powder with a slight fruity odor. It is insoluble in water but soluble in kerosene, oils, and most organic solvents. It can be used as a dust mixed with talc or other diluent or as a spray suspended or emulsified in water or in solution in kerosene, fuel oil, xylene, or other solvent. It can also be used in aerosol bombs, dissolved in a mixture of solvents, one of which, dichlorodifluoromethane (Freon-12), has a sufficiently high vapor pressure at ordinary temperatures to expel the contents of the bomb when the valve is opened.

For combating the body louse, the dreaded carrier of typhus, the soldiers dusted themselves with a powder containing 10 percent of DDT.

During the war bedbugs were controlled by spraying mattresses and beds with a 5 percent solution of DDT in kerosene. Mosquito larvae were killed by a fuel-oil solution of DDT distributed by airplane over marshes. Underwear was proofed against body lice by washing in a xylene emulsion of DDT. For agricultural use DDT is generally applied as a suitably diluted dust or in water suspension as a spray.

The great value of DDT as an insecticide lies in its residual action. DDT is not volatile and not soluble in water. This means that, when sprayed on apples, for example, it does not evaporate and is not washed off by rain but remains as a toxic coating giving protection against insect attack.

The toxicity of DDT to man and domestic animals is low. DDT dust is not irritating to the skin, and millions of pounds have been applied to fruits and vegetables without harm to the consumer. Pharmacologists warn, however, that, despite the fact that DDT has been studied for at least five years, there is still need for more information before a complete appraisal can be made of the hazards involved in its use.

Chlorinated insecticides closely related to DDT are its methoxy analog, $C_{16}H_{15}Cl_3O_2$ (31 percent chlorine), and TDE, $C_{14}H_{10}Cl_4$ (44 percent chlorine). These materials are less toxic than DDT to warm-blooded animals, but they are also less toxic to most insects. The acute oral toxicity to rats of the methoxy analog of DDT is 1/24 that of DDT; that of TDE, 1/10. It should be remembered that acute toxicity bears no relation to chronic toxicity. The methoxy analog of DDT is a kidney poison, and TDE, in addition to being a liver toxicant, appears to have a special predilection for the adrenal glands.

Benzene hexachloride is another old compound only recently found to have value as an insecticide. It was first made in 1828 by Michael Faraday, but was not tested as an insect poison until 1941 in France. At about the same time the English began experimenting with it and discovered that it was highly toxic to many injurious insects. In England it has been called 666 and Gammexane.

Benzene hexachloride, $C_6H_6Cl_6$, prepared by adding chlorine to benzene, contains 73 percent of chlorine. It is easily made, and the raw materials are abundant and cheap. In tests in this country it has proved effective against lice and ticks on livestock, the boll weevil, grasshoppers, and wireworms in soil. Although superior to DDT for these uses, it lacks the residual action of DDT. This chemical has a strong, persistent odor, which is picked up and retained by many fruits and vegetables, and this fact may greatly limit its field of usefulness. In New Jersey last year many potatoes were made unfit for human consumption because too much benzene hexachloride had been added to the soil to kill wireworms. The gamma isomer of benzene hexachloride, which accounts for the insecticidal value of the technical material, has twice the acute oral toxicity of DDT when fed to rats. It is a liver poison.

Chlordane, $C_{10}H_6Cl_8$, containing 69 percent of chlorine, is an American development and is now manufactured by two companies in this country. It is several times as toxic as DDT to house flies and cockroaches, but lacks the long-lasting, or residual, property of DDT. It has come into extensive use by pest-control operators and is employed on a large scale for grasshopper control. Other insects susceptible to chlordane include ants, chiggers, ticks, fleas, mosquitoes, the boll weevil, the plum curculio, and the squash bug. The acute oral toxicity of chlordane to rats is one half that of DDT.

Chlorinated camphene, $C_{10}H_{10}Cl_8$, is made by chlorinating camphene, which in turn is made from pinene, a constituent of turpentine. Its chlorine content is 68.5 percent. This insecticide appears especially promising for the control of insect pests of cotton. Its acute oral toxicity to rats is four times that of DDT.

1,1-Bis(*p*-chlorophenyl)ethanol, or $C_{14}H_{11}OCl_2$, contains 26.5 percent of chlorine. It may be regarded as a combination of chlorobenzene (used in making DDT) and ethyl alcohol. It has given promising results against mites, especially in apple orchards in the Pacific Northwest.

Bis(*p*-chlorophenoxy)methane, or $C_{13}H_{10}OCl_2$, contains 26.3 percent of chlorine. It is related to DDT and also to 2,4-D, the famous weed killer. It has given good results in the control of the citrus red mite, a serious pest of lemons and oranges in California.

Little is known of the pharmacology of 1,1-bis(*p*-chlorophenyl)ethanol and bis(*p*-chlorophenoxy)methane. They should be handled with caution, and spray residues should not be left on fruits or vegetables intended as food for either man or beast.

The other class of new insecticides, the phosphorus compounds, has shown amazing toxicity. No one would have suspected that these organic phosphates would prove so poisonous. Unfortunately, they are highly poisonous to man as well as to injurious insects.

The story of these organic phosphorus compounds is a romantic one. About twenty years ago the Germans started research to develop new and more powerful war gases. Hundreds of compounds

were synthesized and tested on laboratory animals. They were also tested on cockroaches, flies, and other insects to determine their possible value as insecticides. In this way there was developed hexaethyl tetraphosphate, which proved very toxic to plant lice and was used by the Germans, in a formulation called Bladan, as a substitute for nicotine. In the summer of 1945 the Army sent American chemists to Germany to interview German chemists and to uncover German chemical secrets. In this way information concerning hexaethyl tetraphosphate was brought to this country.

Hexaethyl tetraphosphate is easily made by reacting triethyl phosphate, a liquid used in the plastic industry, with phosphorus pentoxide or phosphorus oxychloride. The product is a heavy liquid, which dissolves readily in water and most organic solvents, excepting kerosene. Hexaethyl tetraphosphate is twice as toxic as nicotine to aphids. This fact is of commercial significance, because the demand for nicotine is always greater than the possible supply from tobacco. Hexaethyl tetraphosphate is also highly effective in killing many other insects and is toxic to warm-blooded animals as well, its acute oral toxicity being thirty-five times that of DDT.

Studies by chemists at the Agricultural Research Center at Beltsville, Maryland, have shown that hexaethyl tetraphosphate is a mixture and that its toxicity is due to its content of about 20 percent of tetraethyl pyrophosphate. This compound can be readily made by the same process used in making hexaethyl tetraphosphate, and preparations containing about 40 percent of tetraethyl pyrophosphate are now on the market.

The newest insecticide is parathion. This also was developed in Germany, at Elberfeld, where it was known under the code number E–605. Its secret was brought to this country late in 1945. Parathion has aroused great enthusiasm among the applegrowers in Oregon and Washington because of its ability to kill mites, which are resistant to DDT and most other insecticides. Parathion is also toxic to a wide range of insects that attack other fruits and vegetables. It is at least five times as toxic as DDT to mosquito larvae. It is more stable than hexaethyl tetraphosphate and tetraethyl pyrophosphate, and a spray deposit retains its insect-killing property for several weeks. The disadvantages of parathion are its garliclike odor and its high toxicity to warm-blooded animals. Its acute oral toxicity to rats is seventy times that of DDT; moreover, it damages the colon, and necrosis of the gall bladder has been noted in rats. There are indications that parathion has a cumulative effect.

The prospective user of all these insecticides should keep in mind that they are so new that many questions concerning them cannot be answered. Most of them are highly specific in their action and are ineffective against certain of our worst insect pests; some have offensive odors and may taint foodstuffs; all are poisonous to man and must be applied at such times and in such dosages as to avoid spray residues on fruits and vegetables. Some may cause toxic symptoms if applied to the skin, and the operator who applies them should wear protective clothing and a gas mask. If handled with an understanding of their physical, chemical, and pharmacological properties, however, these new products will constitute valuable additions to the chemical weapons man must employ in his unceasing war against injurious insects.

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TEXTILES THROUGH THE MICROSCOPE

THE microscope is thought of by the average person as an instrument a scientist uses for some such study as the anatomy of bugs. It would seldom occur to him that this same instrument is used, although indirectly, in the improvement of chemically treated textiles, which the customer will eventually purchase in a department store. For example, the housewife examines a piece of cloth for desirable qualities with never a thought of what chemicals or processing went into its manufacture or how the small fibers would look if he could see them.

The microscopist in a textile laboratory is able to observe such things as the fiber shape, the structure of fibers, yarns, and cords, and the penetration of chemicals into yarns and fiber walls so that the chemist may know why his treatment did or did not give good results. Full discussions of the usual methods for such investigations have been published in both textbooks[1,2] and journals.[3,4]

The chemist measures quantitatively the percentage "takeup" of chemicals in a series of samples treated by different methods, but only

through microscopy can he know just how far these chemicals have penetrated into the fiber or yarn. Crease resistance in fabrics may be obtained by impregnating the cloth with any one of several resins[5]. If resin-impregnated cotton yarns are soaked in a solution of a dye that stains the resin and not the cotton fibers and then a thin section is cut, the penetration or lack of penetration can be seen. Figure 1 (*A*) represents a cross section of a pretreated cotton yarn photographed under the microscope to show that melamine resin has partially penetrated the fiber walls; and (*B*) shows that urea formaldehyde has penetrated the yarn only. To obtain these pictures, treated samples were dyed overnight in a 1 percent solution of Kiton Pure Blue V, rinsed thoroughly, and cros sectioned in the Hardy hand microtome,[6] a speci device designed for cutting fibers. Thus, with th technique, samples treated according to vario procedures can be examined to see exactly what h

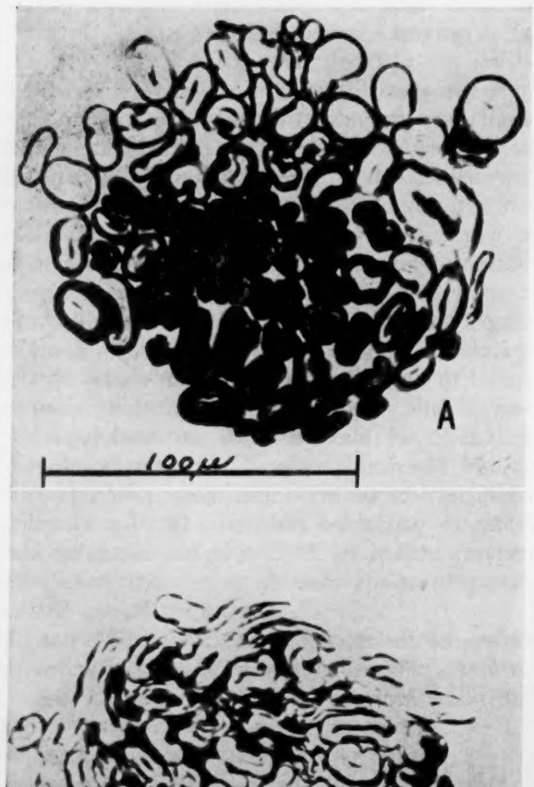


Fig. 1. *A*: Cross section of pretreated cotton yarn in which black areas represent penetration of melamine resin into fiber walls. Some fibers show complete penetration and others are affected around outer edge only. *B*: Cross section of cotton yarn in which black areas represent deposition of urea formaldehyde resin around fibers.

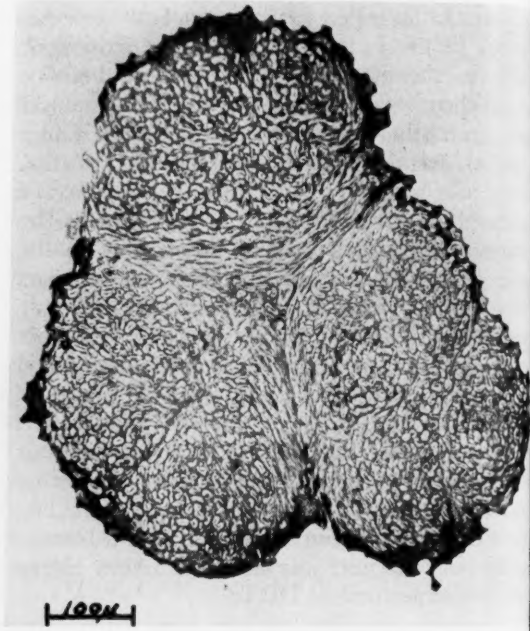


Fig. 2. Cross section of 3-ply cotton tire cord showi adhesion of black rubber stock to edge of cord. Each p contains 4 single yarns.

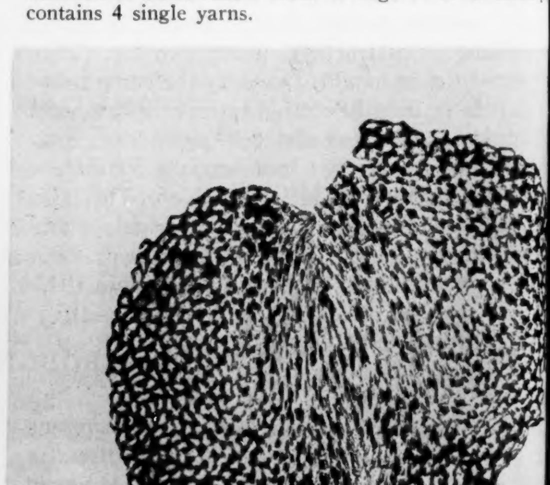


Fig. 3. Cross section of 2-ply rayon tire cord show penetration of latex into cord. Black areas represent loc tion of latex.

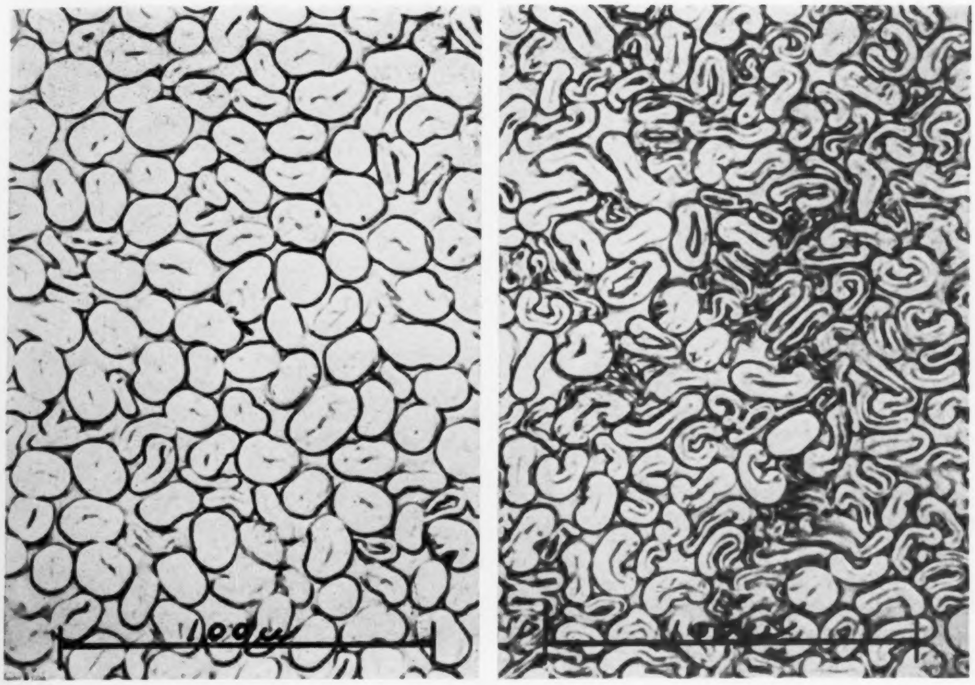
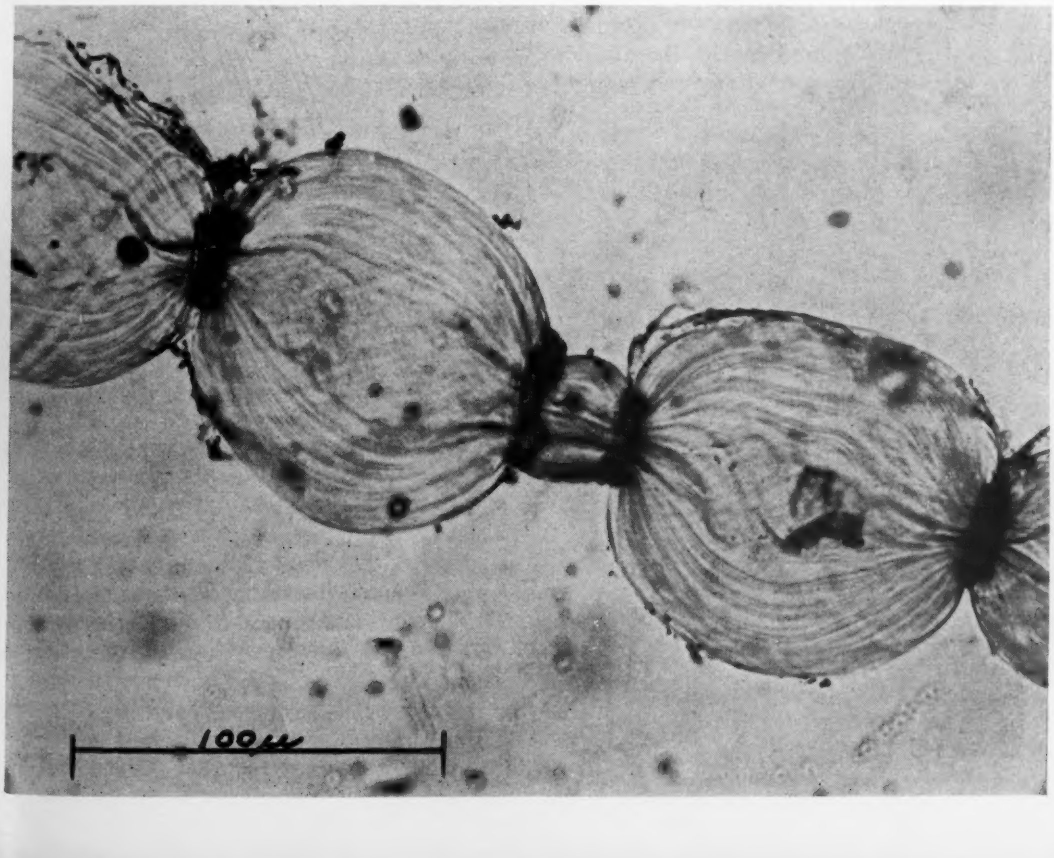


Fig. 4. *Left*: Cross section of well-mercerized Empire cotton. *Right*: Cross section of untreated Empire cotton.

Fig. 5. Longitudinal view of cotton fiber swelled with cuprammonium hydroxide to show ballooning. Inside the balloons may be seen the layers of cellulose composing the secondary wall.



happened during each treatment. The method of application that gives the best results can then be selected.

Textile microscopy can also be applied in the study of tire cord construction, adhesion, and impregnation. Rubber adhesion may be determined visually by the microscopical examination of a cross section in which the black rubber stock around the edge of the cord is easily discernible (Figure 2). Latex penetration presents a problem of its own because of the lack of color of this substance; however, by using a staining technique developed at the Southern Regional Research Laboratory,[7] it is possible to show the exact location of the latex *in situ* in the cord. Figure 3 is a cross section of a 2-ply rayon cord showing penetration of latex into the center of each ply.

As a further illustration, the microscope has been useful in studies of mercerized cotton, known principally because of its commercial use in sewing threads, socks, and fine fabrics. This chemical process, which involves treatment with 18 percent sodium hydroxide under tension, swells the fibers and leaves them lustrous. To determine whether a sample of cotton has been well mercerized, it is cross-sectioned and examined under the microscope. If the treatment has been effective the fibers will appear to be virtually round (Fig. 4, *left*) as compared with irregularly shaped fibers in an untreated sample (Fig. 4, *right*).

When the chemist is familiar with the make-up of cotton he can often predict the results of his

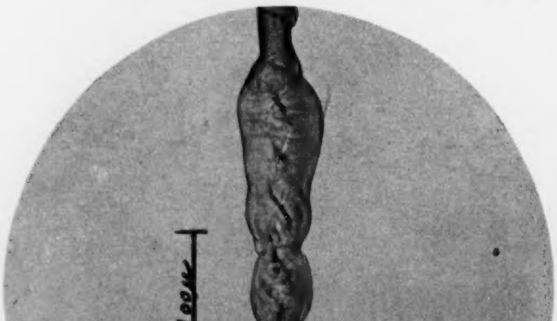


Fig. 6. Longitudinal view of partially carboxymethylated cotton fiber swelled with water to show ballooning similar to that seen in Fig. 5. Layers of secondary wall are not visible since fiber appears to be fused.

treatment before applying it. The microscope aids in studying the internal structure of cotton.[8,9] For this purpose, fibers are swelled with cuprammonium hydroxide, which causes the primary wall of raw cotton to peel back and the secondary wall, consisting of layers of cellulose, to swell out and form balloons. Figure 5 is a photomicrograph of a cotton fiber in which the primary wall, under pressure of the swollen cellulose, has burst and formed constricting collars, or bands. This microscopical technique reveals the cellulosic layers composing the secondary wall, spiraled bands around the outside of the fiber, and, sometimes, reversals in the direction of this spiral.

With the microscope it is possible to study such chemical treatments as the partial carboxymethylation of cellulose,[10] which produces quickly swellable cotton fibers to be used in cloth where rapid absorbency is desired. Figure 6 shows a partially carboxymethylated fiber swelled with water to test the effectiveness of the treatment used. Although the secondary wall of the fiber is distended as in Figure 5, the lamellae are not visible because the chemical and physical properties have been altered by the treatment.

Microscopical investigations have been useful as a check in differential dyeing studies of thick- and thin-walled fibers.[11] According to this dyeing technique, when a mixture of mature (thick-walled) fibers and immature (thin-walled) fibers is dyed in a special solution containing both red and green dyes, the mature fibers absorb the red dye and the immature fibers, the green. The microscopist separates the sample into the two colors and examines the fibers in cross section. For all the varieties of cotton examined, the microscopical test has checked with the dyeing test. Figure 7 (*left*) is a photomicrograph of a cross section of mature fibers (dyed red) and (*right*) of a cross section of immature fibers (dyed green).

Problems in fiber identification are frequently solved with little difficulty when the sample is examined in cross section under the microscope. Each type of fiber has its own characteristic shape and, therefore, an unknown sample may be identified by comparison with others of known origin. Figure 8 is a cross section of a yarn purported to contain all wool fibers. Upon examination, however, it was found that the cloth contained a mixture of wool, rayon, and cotton. Of interest also is the fact that it was possible to detect the presence of reworked wool because of the wide variety of colors represented in the center of these fibers, indicating their previous use in lighter-colored textiles.

The role that the microscope plays in the im

Fig. 7. *Left*: Cross section of mature (thick-walled) cotton fibers dyed red in differential studies. *Right*: Cross section of immature (thin-walled) cotton fibers dyed green in differential dyeing studies.

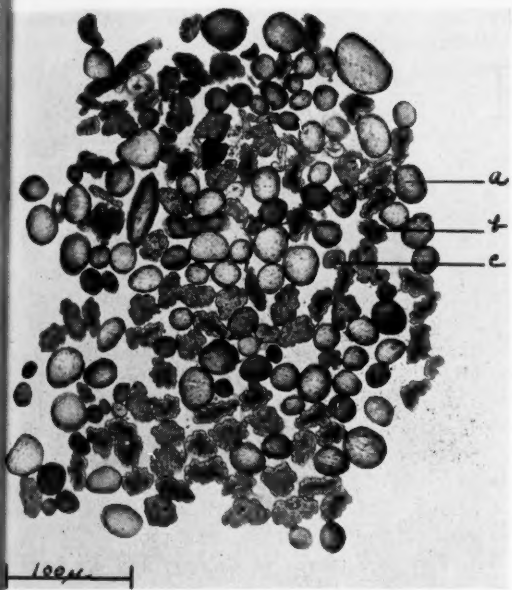


Fig. 8. Cross section of mixed yarn containing (*a*) wool, (*b*) rayon, and (*c*) cotton fibers.

provement of commercial textiles is varied and only briefly outlined in this limited space. There are many other applications of microscopy that play an important part in the commercial manufacture of chemically treated textile materials. Fiber microscopy is not used to study chemically treated materials only, however, but may be applied to such related branches of research as genetics, physical testing, fiber and yarn processing, and the inspection of damages caused by weathering, aging, and mechanical wear. This field of research offers unlimited opportunities for the fiber microscopist to assist first the chemist and ultimately the manufacturer in the development of improved materials for consumer use.

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BOOK REVIEWS

THE STUMBLING CLIMB

Science and its Background. H. D. Anthony. 304 pp. $3.00. Macmillan and Co., Ltd. London.

IN RECENT years many histories of science have appeared, dealing with one branch of science or one period or a single country. Similarly, historians have recognized the importance of science in the development of human knowledge as a whole. The author's claim to a new approach to the subject is based on his effort to bring these two lines of investigation into intimate conjunction, carrying them along together so that scientific discoveries become an integral part of the general stream of man's development through the ages.

In the earlier part of the volume, the research, teachings, and discoveries of about fifty outstanding men are set forth in the social and political background in which they arose. Thales, the philosopher, Hippocrates, the great physician, Leonardo da Vinci, Aristotle, and Galileo may serve as examples of the author's clever selections to show how from the beginning of the human struggle to understand the world, one after another great thinker has expanded the field of science with contributions vital to further progress.

Progress was by no means at a uniform rate. There were times when science languished in the doldrums of confusion. Then some brilliant genius invented an instrument or devised a new method that threw a clear light on enigmas and mysteries, and made the pathway easier. There is a real excitement in traveling again over the road on which man stumbled forward for centuries. Such a simple matter as the Arabic system of numbers made elaborate computations, theretofore impossible, easy and rapid. What if today we tried to multiply DCC by XIX in the old Roman notation? Then in the nick of time Copernicus upset the Ptolemaic, or geocentric, theory of the universe, thus clearing up one more mystery; or Harvey demonstrated the circulation of the blood; or Newton discovered the law of gravitation; or Lavoisier laid the foundations of chemistry. Even the alchemists were on the right track on the possibility of transmutation of the original elements. Today the atomic investigations have set the transmutation of uranium among the items of information in the daily press.

The name of Pasteur is immortalized in the process of rendering milk safe to drink. Lord Lister slew the dragon of septicemia. Simpson and Morton brought the blessing of anesthesia to suffering man.

The author takes the reader step by step along the road man has traveled, showing how the thousands of inquiring minds have paved the way to the myriad contrivances of the modern world. And the end is not yet. Each discovery leads to another, the rope by which man has climbed consisting of three strands—knowledge, action, and vision. Each discovery is quickly translated into action and practical use, and also sets men to dreaming of further advances.

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TAGGED ELEMENTS

The Use of Isotopes in Biology and Medicine. A symposium. xiv + 445 pp. Illus. $5.00. University of Wisconsin Press. Madison.

THE use of isotopes in biology is about twenty-five years old, in biochemistry only fifteen, and in medical diagnosis and therapy about ten. Hence the field is very new, standard practice is in the process of development, and the number of workers acquainted with the techniques few.

The present book, really a collection of related papers by notable pioneers in the production and utilization of isotopes, is designed to educate not only the novice about to begin his investigation, but also the expert who wishes to keep himself informed as to the progress being made by his colleagues in related fields of endeavor. It is recommended to the nuclear physicist or isotope chemist who wishes to know how his products are being utilized in order that he may plan the direction of his future efforts, and to the biologist, biochemist, and medical man who wishes to make the most efficient use of this new tool.

The Use of Isotopes in Biology and Medicine begins with a short Preface by Perry W. Wilson and an address of welcome by William A. Middleton, University of Wisconsin, where the symposium was held. The first paper of the book proper is an excellent résumé by Hans T. Clarke, of Columbia University, of the Historical Background of Isotopes in Bio-Chemistry. It will be recalled that Dr. Clarke was associated with Dr. Urey in the early days of the separation of heavy water and that he also played an important role in the penicillin research program during World War II.

In the next paper—Separation of Stable Isotopes—the reader is given a brief but thorough course in the cascade diffusion method of Hertz, the chemical separation methods, and the thermal diffusion methods of Clausius and Dickel. This is followed by The Preparation of Radioactive Isotopes, by Glenn T. Seaborg, of the University of California.

Next, Paul C. Aebersold, formerly of the Naval Research Laboratory, Anacostia, D. C., now Chief of the Isotopes Branch, U. S. Atomic Energy Commission, discusses Recent Developments in the Avail-

ability of Isotopes. This is an important chapter for those who contemplate work with a new isotope and who need to know what quantities of this isotope can be obtained.

The fifth, sixth, and seventh papers deal with the detection and measurement of isotopes. These are The Detection of Stable Isotopes, by Alfred O. Nier; Fundamental Principles of the Detection and Measurement of Radioactivity, by Charles D. Coryell; and Assay of Radioactive Isotopes in Biological Research, by Martin D. Kamen. The eighth paper—Preparation of Compounds Containing Isotopes, by Donald B. Melville—ends what might be considered the preliminary part of the book, on history and general techniques.

The middle section, consisting of papers nine to sixteen, inclusive, is of special interest to physiologists and medical investigators. The first five of this group deal with metabolic processes. These are: Studies on the Metabolism of Proteins, by David B. Sprinson; The Use of Isotopes in the Study of Intermediate Carbohydrate Metabolism, by Harlan G. Wood; The Intermediate Metabolism of Lipids, by Konrad Bloch; Tracer Studies on the Metabolism of Mineral Elements with Radioactive Isotopes, by David M. Greenberg; and Application of Radioactive Iodine to Studies in Iodine Metabolism and Thyroid Function, by I. L. Chaikoff and A. Taurog. Papers on Medical Applications of Radioactive Tracers, by Joseph G. Hamilton; Therapeutic Use of Radiophosphorus in Polycythemia Vera, Leukemia and Allied Diseases, by Byron E. Hall; and Treatment of Thyroid Disease by Means of Radioactive Iodine, by Saul Hertz, are of especial interest to the specialist or general practitioner in clinical practice. By way of an aside, Hall's paper should be of some interest to hematologists investigating the cause and treatment of radiation illness as seen in patients and animals exposed to atomic bomb ionizing radiations.

Health Hazards in the Use of Radioactive Isotopes, by William F. Bale, can be classified under the heading of Health Physics. It is an able condensation of many important parts of the late D. E. Lea's book *Actions of Radiations on Living Cells,* plus a few additions. The next paper, by James J. Nickson, Measures for the Protection of Personnel and Property, also belongs to Health Physics, with a sanitary engineering angle.

International Aspects of Atomic Energy, by Harold C. Urey, takes us out of the realms of science into those of politics. This paper should be read by everyone. The last paper, Comments on the Development of Atomic Energy, by Farrington Daniels, is a general survey of some of the problems to be expected in the course of the development of atomic energy.

A listing of other papers on special topics ends the book.

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ABC

The Alphabet: A Key to the History of Mankind. David Diringer. 607 pp. Illus. $12.00. Philosophical Library. New York.

THE first third of this book deals with the history of the nonalphabetic scripts in roughly chronological order: cuneiform writing; hieroglyphic writing; the Cretan scripts; the undeciphered script of the Indus Valley civilization; the scripts of the Hittites; Chinese writing; the ancient scripts of the Americas; the script of Easter Island; other ideographic scripts of certain Asiatic, African, and American peoples; syllabic systems of writing; and certain systems the author calls "quasi-alphabetic"—for example, the early Persian cuneiform, or the inscriptions from Meroë in southern Egypt. The rest of the book is devoted to the main theme: the development of the alphabet itself. Another book promises to deal with the various problems of paleography, or "writing" as a whole, including handwriting. H. J. Uldall (Speech and Writing, *Acta Linguistica,* 1944, 4.11–6) has recently shown the possibilities a systematic study of writing can offer.

There are few comprehensive studies on this subject in the English language since Isaac Taylor's fundamental contribution of 1883. But this book does much more than merely fill a gap: it is bound to stand as the most authoritative treatment of the history of alphabetic writing for a long time to come. This is because the book is extraordinarily scholarly and exhaustive. It is, incidentally, also quite exciting reading. Diringer very seldom engages in speculation, but includes only material he can verify from different sources. His bibliography, though not complete, is useful and well arranged. It is too bad that the author found it necessary to cater to that fictitious individual, "the general reader," here and there, particularly in omitting the diacritic marks that give us the precise pronunciation of the written symbols.

The history of the alphabet illustrates at least three cultural processes: diffusion, independent invention, and "idea diffusion" (Kroeber's stimulus-diffusion). For this reason, the book will have special interest to scholars dealing with the dynamics of culture change. The author adduces much evidence to prove that most alphabetic writings are derived from a single source. The locus of invention was most likely Syria-Palestine, in the second millennium B.C. Of course the scripts of Egypt, Babylonia, and Crete, in all probability, exerted some influence in the process.

The jungle of material connected with the story of the alphabet and its descendants must have been very difficult to organize into logical divisions. This was accomplished with considerable skill, however. From the problem of origin, the author passes to the South Semitic alphabets of Arabia, the beginnings of which still constitute an open question. He next examines the Canaanite (early Hebrew and Phoenician) scripts, the Aramaic ones, and offshoots of the latter used by

non-Semitic groups. One of the most complicated and difficult groups of problems relates to the alphabets of India and southeast Asia, to which the author devotes more than a hundred pages. We approach home base when we reach the Greek alphabet, the main offshoot of which was the Etruscan alphabet; its descendant, the Latin script, in turn, led to the development of all the modern alphabets of western Europe.

The many illustrations add much to the fascination of the book.

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SOCIAL INTEGRATION

Society as the Patient: Essays in Culture and Personality. L. K. Frank. xiv + 395 pp. $5.00. Rutgers Univ. Press. New Brunswick, N. J.

INTEGRATION is as much an imperative in academic thought and science as it is in personality if social progress is to be sustained. For a quarter of a century, Lawrence Frank has pioneered this point of view in his insistence that a psycho-cultural approach to our problems—utilizing the confluent skills and findings of psychology and psychiatry, anthropology and sociology—is essential if adequate solutions are to be found. The present volume of thirty essays, reprinted in essentially their original form from the many professional journals to which he has contributed, represents the wide range of interest to which he has applied his principles of confluence and synthesis. The fields range from economic to social psychology, from education to the speculative orientation of the arts and sciences.

Too many people for too long have operated in the belief that we are all a product of the culture in which we live, as if that living matrix were immutable. Frank's most consistent argument—which strikes at the heart of all temporal power and vested interest intent upon preserving the *status quo* for their own sake—is that culture is plastic and elastic. It holds the hope "of a culture to serve human needs and values. . . ."

Frank is revolutionary only in so far as being thought-provoking is revolutionary. As a social scientist, he is a democratic evolutionist firm in the belief that the individual, too often sacrificed to social organization through his own doing, need not be so sacrificed. Freedom from the social "plaster cast" does not demand shattering that cast, but rather understanding how it came into being, the individual's relationship to it, the possibility of substituting a lighter, more durable, more useful cast. In the beginning man, out of his need for him, made God; and he has been changing, swapping, and substituting for him ever since. This is something of our relationship to culture.

In none of these essays has Frank attempted to package any of the problems about which he writes or to establish any formulas for their solution. Rather, in the language of experimental science, he points the way to, and provides us with, leads. For those who have missed the provocativeness of Dr. Frank's individual contributions, this volume will be most welcome.

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PATTERN OF ALIENATION

A Mask for Privilege. Carey McWilliams. xiii + 280 pp. $2.75. Little, Brown. Boston.

THE thesis of this book is that anti-Semitism is inextricably related to our capitalist society, that it was instigated during the last century by the "industrial tycoons" as a diversionary, scapegoat technique to defend their own privileged positions and to conceal their egregious exploitation, and that it developed into an antidemocratic force, following the consecutive pattern of social, economic, and political exclusion. Driven inevitably to marginal occupations and status, the Jew thus becomes uniquely vulnerable to group hostility. Anti-Semitism, therefore, will ultimately cease only in a noncompetitive, planned society. Although the societal emphasis of the book is a valuable antidote to much of the loose psychologism permeating many of the works today purporting to deal with "prejudice," it rides its economic determinism too hard in the face of—and to the neglect of—other historical factors.

1) Mr. McWilliams never poses these crucial questions: Why in 1877 did the conspiring "industrial bourgeoisie" (rather simplistic historiography) choose the Jew as scapegoat? Why did otherwise sober men of goodwill among *all* classes, trades, and professions find it easy then, as now, to believe the most fantastic theories and canards about him? Why is it that, although other racial, religious, and national minorities have also suffered discriminations, the Jew remains unique among Toynbee's "penalized minorities" of vestigial antiquity? The answer antedates industrial capitalism; it is to be found in organized Christianity, which during its beginnings developed a theological corpus concerning the Jew that not only provided the rationale for official oppression but for all the subsequent iniquities associated with Jewish stereotypes. Before the advent of the Christian Church the Jews had, like others, incurred dislike whenever they participated in economic, cultural, and religious rivalries with different peoples; but after that they became a subject group, deprived of the citizenship they had enjoyed under the Roman Empire. More than that, they were viewed—according to patristic falsifications of history—as the crucifiers of Christ, cursed by God for having betrayed his laws and condemned to wander the earth until the required remnant had been converted to Christ's vision. It was in this period that the concept of the Jew as an "alien body"

was created, a concept which provided a *continuing* base for the economic, social, political, and psychological accretions of anti-Semitism later grafted upon it and which also explains why those multiple and contrary accusations leveled at the Jew by his enemies to this day are so plausible to them. Being what he is, a Jew is simply capable of anything! Incidentally, in view of the fact that McWilliams' pet thesis has long been subscribed to by "bourgeois," let alone Marxist, investigators, it is difficult to account for his statement that "the inadequacy of social theory in relation to this crucial problem is a scandal for which every social scientist in the U. S. should feel ashamed."

(2) Although the author outlines the customary libertarian program of civil, legal, political, and economic weapons to combat prejudice and discrimination, the underlying assumption conveyed is that reformism alone will not suffice. What is needed is a democratic, noncompetitive society, one ostensibly approximating the success of the USSR where "the physical and economic security of the two million or more Jews may be taken for granted." No one need argue at this late date that scapegoatism is correlated with economic insecurity, social conflict, and political instability. Furthermore, whether any society, no matter how enlightened, can ever completely cope with the problem of emotional deprivation so that no individual experiences aggressive impulses remains for the future. What we cannot permit to pass at this point, however, because of the important relation between societal structure and intolerance is McWilliams' perpetuation of another Soviet myth.

Contrary to popular opinion, the USSR is not free of anti-Semitism. That the voluble defenders of Russian policies throughout the world point to a democratic constitution instead of to daily realities as incontrovertible evidence is another example of what Silone in another connection has called legalistic "cretinism." Space does not permit any elaboration of this much-misunderstood problem; but the interested reader may consult such authoritative sources as Schwarz, Nomad, and Lestchinsky, as well as the reports of refugees and displaced persons and of the Worldover Press concerning the growth of anti-Semitism within the USSR and its satellite nations. What exists at best in the Soviet Union is ethnic equality; this means that the Jews, like other minorities, are subject to similar privileges and indignities. Ethnic democracy, or the political freedom of a group in its relation to the state, the Jew certainly does not enjoy.

Finally, since McWilliams' cure for anti-Semitism lies in a radical transformation of our society, to be effected in large measure, no doubt, by a political party, one wishes that he were not so nebulous on this subject and that he would for sake of programmatic clarity indicate what existing organization is most capable of furthering his socialist objective.

GEORGE KIMMELMAN

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PHYSIOLOGY FOR BEGINNERS

The Machinery of the Body. (3rd ed.) Anton J. Carlson and Victor Johnson. xxi + 639 pp. Illus. $4.50. Univ. of Chicago Press.

THIS book, the first edition of which appeared in 1937, is designed to serve the beginner in the science of physiology. The authors are eminently well qualified for the task. Dr. Carlson, who is renowned for his work in experimental medicine, is professor emeritus of physiology at the University of Chicago. Dr. Johnson, who was at the time of the first edition a member of the same department, is now director of the Mayo Foundation for Medical Education and Research.

With the background available to the authors it is not surprising that this textbook should be accurate and remarkably inclusive. Constantly emphasized are such important aspects of science as the close relationship of physiology to chemistry and physics. The importance of the experimental method is stressed throughout. As befits the orientation of scientific thought today, deficiencies of knowledge, and problems requiring further research, are frequently pointed out.

Long familiarity with scientific terminology has led the authors to assume an unusually large vocabulary even for the college level. It is undoubtedly true that the vocabulary is expanded and enriched by stretching for words; nevertheless, some simplification in this respect might not be amiss, and the incluson of a glossary of terms for the scientifically untrained reader would greatly facilitate reading the book.

From a mechanical point of view, with the possible exception of illustrations, this book is well done. The type is clear and large. There are few misprints—"mecury" on page 193 is one spotted by this reviewer.

There is no doubt that this is one of the best books available for the intelligent reader who desires to learn something of the workings of the human body. Any educated person cannot help but enjoy reading this book. Indeed, it contains much of value for the scientist who already has some familiarity with most of the topics presented.

EDWIN P. JORDAN

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THE SOCIAL INSECTS

Our Enemy the Termite. (Rev. ed.) Thomas Elliott Snyder. xiii + 257 pp. Illus. $3.50. Comstock.

SOME advances have been made in the fields of termite biology and control since the first edition of this book appeared in 1935. The revised edition presents much of this new material, especially that in the field of control. Many new and excellent illustrations have been added.

The first section of the book, dealing with the biology, taxonomy, and paleontology of the group is

generally good. Dr. Snyder has changed his position on the determination of castes to the inhibition theory, which is in line with the most recent experimental findings. I feel, however, that there are several portions of the discussion that do not present the most recent aspects of caste development and where the presentation of the facts is confusing. I refer mainly to the sections dealing with Reproductive Forms and Intermediate Reproductive Forms. It seems unfortunate that the more recent view regarding the reproductive forms, which is strongly supported by experimental evidence, is not also presented. This view holds that there is only one type of true reproductive caste, the alate (the "first form" reproductives of Snyder). All other individuals functioning as reproductives are supplementary forms, arising from immature nymphs, either wing-padded or apterous (the "second form," "third form," and "intermediate" reproductives of Snyder).

The second section of the book presents an honest picture of termite damage and critical suggestions for the prevention and control of termite attacks. It includes a discussion of the new insecticides, as well as those which have been utilized for many years. The keys for the identification of the genera and subgenera of the Isoptera occurring in the United States may prove difficult for the uninitiated. The usefulness of the book is greatly increased by the extensive glossary, which should be a requisite for all such volumes.

The value of the book will be self-evident. It has seemed most important however, to point out a few weak points upon which those wishing to inquire further may be well advised to refer to the work of the investigators mentioned in the text.

FRANCES M. WEESNER

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THE ANATOMY OF PHILOSOPHY

The Basis and Structure of Knowledge. W. H. Werkmeister, xi + 451 pp. $5.00. Harper. New York.

THE subject indicated by the title of this book is one of the most obscure in the vast literature of philosophy. Let it be said at the outset that the author has labored earnestly, and always with scholarly ability, to clarify the topics treated with due reference to the developments of the last century in the physical sciences and mathematics. The point of view of this volume is indicated in the introduction, in which the author finds that the position of the "logical positivists" is on the wane. The present book consists chiefly of an elaboration of certain ideas previously expressed in the author's *Philosophy of Science* and other writings. The contents are organized into four principal sections as follows: Language and Meaning; Truth and the World About Us; Formal Knowledge; Empirical Knowledge. The entire second half of the book is devoted to a discussion of the nature of mathematics and scientific method, laws, and principles.

As may be surmised from the Table of Contents, all the knottiest problems of philosophy, and especially of epistemology, are encountered in this volume. An adequate discussion of the author's handling of a single one of them would go far beyond the limits of this review. Suffice it to say that, in my opinion, the basis for the interminable difficulties with which this book struggles is the reluctance to abandon the ancient dualism of "mind" and "matter," to use the traditional terms. If one wishes to adopt this dichotomy (however disguised), certain consequences doubtless follow and there is little reason to quarrel about them. It becomes rather a practical question of what it shall profit us in the solution of certain problems of living on the earth to adopt positions of the type reflected in such passages as, for example, the following:

> The moral and social obligations to which scientists must and do submit transcend the realm of empirical research to find their justification and sanction, not in laboratories and scientific methods, but in a realm of values—human and humanitarian—which can neither be scientifically analyzed nor reduced to functional equations (pp. 331–2).

> Any science, therefore, which is concerned with the characteristic features of "mind" as they are revealed in the "modes" of experience deals with a subject matter which is not in the same universe of discourse with the content of experience which is the subject matter of the natural sciences. Hence, to the extent to which psychology is concerned with "mind" in the sense here defined, it indicates a new dimension of investigation and can never be included in the integrated and "closed" system of the natural sciences. The categories of the "external" world and the principles and "laws" which integrate the content of experience, are not of the type required for the understanding of the "*modes*" *of experiencing* that content (p. 418).

It is true that the author definitely disclaims (p. x) any intention of dealing in this volume with the social sciences (which he promises to do in a future work), and these few passages in a book devoted mainly to other subjects should, therefore, not be too greatly emphasized. I quote them chiefly to characterize the viewpoint of the book rather than to imply that the contents are mainly on this subject. Also I believe that the viewpoint reflected in these passages underlies in a fundamental way all the obscurities and inadequacies which characterize the whole volume. In the final analysis, science and philosophy are both a kind of social behavior, and the epistemological problems that have always bedeviled the subject of philosophy can, in my opinion, be resolved only when a more adequate science of human social (including verbal) behavior is developed. But if we assume to begin with the impossibility of an empirical science of this subject matter, then I fear philosophers will

continue to pursue epistemological problems in a perpetual circle.

The fact remains that the author has written a scholarly treatise from the viewpoint he has elected to espouse. Those who find this viewpoint a profitable and an illuminating way of looking at, and verbalizing about, certain problems of philosophy will doubtless find this an excellent text. Nor should the critical remarks made above be allowed to distract attention from the wide range of interesting material and reasoning the author has assembled and, of course, the many excellent discussions with which, I suppose, nearly all would agree. There is an excellent bibliography and an index.

George A. Lundberg

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EVERYMAN'S COMSTOCK

The Insect Guide. Ralph B. Swain. xlvi + 261 pp. Illus. (by Susan N. Swain). $3.00. Doubleday. Garden City, N. Y.

SELDOM does one have the pleasure and satisfaction of reading a book so well suited to the purpose for which it was written as *The Insect Guide.* Dr. Swain begins by giving in tabular form the differences between the Insecta and the other arthropod groups—Palaeostraca, Crustacea, Arachnida, Diplopoda, and Chilopoda—all of which are figured. The Onychophora are omitted, probably because they are not regarded as arthropods or because they are not found north of Mexico. Following this are a brief but well-written and adequate account of insects in general, and another of their structure, the latter accompanied by good figures of their essential features.

The main body of the work consists of notices of the more important families of insects in North America north of Mexico. The selection of families and of the examples chosen to represent them is excellent. An enormous amount of information is given, written in a condensed yet clear and attractive style. This information has evidently been very carefully checked, for there are unusually few statements to which anyone could take exception, and these are chiefly in the account of the butterflies. The nomenclature has been brought thoroughly up to date.

The illustrations are exceptionally good, from the technical as well as from the artistic viewpoint. A few of the colored figures were prepared from faded or discolored museum specimens, which is inevitable, and in some the color reproduction is not too good, which is not the fault of the artist. In the figures of butterflies and moths there are a few minor inaccuracies—for instance, in Figures 83a and 84 the forewings are too pointed—but these are negligible. Figure 83b shows both *Colias philodice eurytheme* and one of the yellow subspecies.

The section on collecting, preserving, and studying insects is comprehensive, well written, and well illustrated. An interesting innovation is the tabulation of the insects, with figures, on the end papers.

Dr. and Mrs. Swain evidently devoted a vast amount of time and thought to the preparation of this book. It is no easy task to gather such an immense amount of material and then to sift it and present it in such excellent form. They are to be congratulated on having produced a book that represents a notable contribution to the literature on popular entomology.

Austin H. Clark

Smithsonian Institution

LITERARY FLIGHTS

Voyages to the Moon. Marjorie Nicolson. xii + 297 pp. Illus. $4.00. Macmillan. New York.

JOHN WILKINS said in 1638, "there are four several ways whereby this flying in the air hath been, or may be attempted. . . . By spirits or angels. By the help of fowls. By wings fastened immediately to the body. By a flying chariot." The first part of Dr. Nicolson's scholarly and amusing book discusses the literature of man's attempts to fly from the earliest recorded Chinese and Greek legends; Daedalus and Icarus are familiar to everyone.

The "new astronomy" of the early seventeenth century, the turn from the old superstitious way of thinking toward a more nearly scientific way, engendered a whole new literary trend. Science does stimulate "the minds of writers of fiction." Then the tales of flights—cosmic and terrestrial—drew away from the supernatural voyages such as Kepler's *Somnium* and Francis Goodwin's *Man in the Moone.* Bacon, Wilkins, and others wrote of attempted flights by artificial wings, and such satirists as Samuel Johnson in *The Dissertation on the Art of Flying* made merry. The idea led gradually to the "flying chariot" enthusiasm of the eighteenth century. In 1705 Daniel Defoe in his *Consolidator* played with the "theme of a world in the moon." Swift's Flying Island in the third book of *Gulliver's Travels* marks the "literary climax of cosmic voyages by means of flying chariots."

Later in the book, Dr. Nicolson shows what "certain fanciful, whimsical, sometimes poetic minds made of the planetary voyage." There is "Endymion's way, by rapture in sleep, or a dream" and Cyrano's "translation of Adam" and "the idea of the separation of soul from body" in Gabriel Daniel's *A Voyage to the World of Cartesius.* In mid-eighteenth century Voltaire published his *Micromégas* which may be the parent of today's pseudo-scientific stories of interplanetary activities. For finale, there is Carroll's *Alice in Wonderland,* which, we never before realized, is a cosmic voyage. Jules Verne, H. G. Wells, and other "moderns" appear in the epilogue.

The book is the light avocational work of a well-known educator of distinction. The material was collected over a period of years while the author was

actively engaged in more serious, though related, matters. It has an extensive bibliography and complete index but is not intended to be an exhaustive treatment of the subject. It is meant to be entertaining, and it succeeds in being informative as well.

MARJORIE B. SNYDER

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BRIEFLY REVIEWED

Mathematik, Logik und Erfahrung. Victor Kraft. 129 pp. $2.40. Springer-Verlag. Vienna, Austria.

Most of what Professor Kraft has to say will be familiar to English-speaking students of the philosophy of mathematics. The author is particularly concerned with the empirical validity of arithmetic and geometry, and likewise for logic. He also attacks "conventionalism," this in the sense, for example, that it is a pure convention whether empirical space is Euclidean or non-Euclidean (Russell). Possibly the matter in dispute is one of those things in which no meaningful conclusion can be reached. This, however, does not detract from the interest of the author's presentation.

E. T. BELL

California Institute of Technology
Pasadena

Making Friends with Birds. A. F. Park. xi + 216 pp. Illus. $6.00. Chatto & Windus. London. Macmillan. New York.

This book is not a contribution to technical knowledge of birds, but is concerned with the author's adventures with various common English birds, and his close-range observations of them. Unlike most photographers, he has preferred to work with birds for which no "blinds" or "hides" were needed. This has resulted in the absence from his book of the shyer, more timid, and hence less well-known, species that many photographers seem to consider more interesting or, from the standpoint of photography, more challenging; but it has given him the opportunity of making more extended and more intimate recordings of his subjects. The book is written from the viewpoint of the photographer approaching birds as material, rather than that of the naturalist using the camera as a field tool. The photographs are superb, and the observations in the text are remarkably good, especially when one considers that pictures and not facts were the author's prime goal. Like most popular nature books, there is an element of sentimentality and anthropomorphism in the presentation of the data, but less so than in many books written by men who were primarily naturalists and only secondarily manipulators of cameras.

Some 25 species of birds are treated and pictured in as many chapters. Following these are two chapters dealing with what the author calls the "photographic aspect," in which he gives advice, based on his experience, to prospective photographers of birds' nests, of fledgling birds, and of adult birds, and the differences in the problems each of these categories presents. He ends by giving in tabular form the camera, lens, exposure, f no., emulsion, filter, and lighting used in each of the 180 photographs in the book.

HERBERT FRIEDMANN

U. S. National Museum
Washington, D. C.

The Farmer's Handbook. John M. White. xvi + 44 pp. Illus. $4.95. Univ. of Oklahoma Press. Norman.

I haven't read this book from cover to cover, for it is not the kind of book that you read that way. I have, however, examined it rather carefully and find that it contains a great wealth of information. Naturally, in view of the size of the book and the number of subjects on which it furnishes information, it cannot go into great detail. On a number of subjects on which I have had occasion to check the statements made from the standpoint of accuracy, I have found the information reliable. The book is well indexed, and I think it is likely to be well received by those who may wish a reference book of this type.

V. R. GARDNER

Experiment Station
Michigan State College

The Rescue of Science and Learning. Stephen Duggan and Betty Drury. 214 pp. $3.00. Macmillan. New York.

In 1933 the Emergency Committee in Aid of Displaced Foreign Scholars was organized to assist those scholars who were being driven from German and other European laboratories, libraries, and lecture halls by the mounting fury of Nazi intolerance and persecution. The work of this Committee, directed by the two authors, who are therefore competent to write its history, ended in 1945, and this book is the record of its accomplishments. It is a record in which the United States, its universities, its foundations, and a host of individual contributors may well take pride.

Several thousand scholars were forced to flee from Germany and other Nazi-dominated lands, but not all of these came to the United States. Of those who did seek refuge here, 335 were directly aided by the Committee. This final report therefore tells not only the administrative history of the Committee but also attempts to describe the adjustment of the displaced scholars to their new environments, their contributions to the war effort, and their fields of specialization. It lists the institutions where they were invited to work, together with much other interesting data.

The dry crusts of statistics have been made palatable by being dipped in the milk of human kindness, and the authors and the members of the Committee are to be commended for an inspiring account of a great humanitarian enterprise.

MORRIS C. LEIKIND

Library of Congress

Half Hours with the Great Scientists. Charles G. Fraser. xx + 527 pp. Illus. $6.00. Reinhold. New York.

In recent years there have been published a number of histories of science, some of which have called themselves "stories." For the book under review this title is quite appropriate, for in it the humanistic element is predominant; emphasis is laid upon the attitude of the scientific discoverers—their aims, their points of view, and their methods. The book contains many illustrations, of which thirty-eight are portraits. Moreover, the text contains frequent flashes of humor.

This does not lead to a neglect of the scientific side of the subject. The fundamental facts and principles of physics are classified under five headings, and in each class special attention has been given to such features as might appeal to beginners in physics. As a result, the book as a whole can be recommended as excellent collateral reading for students in physics throughout their whole course of study.

As an instance of some of the interesting things to be found in a study of the scientific methods available to the ancients, the author cites the Greek system of numerals, and gives an example of what we would call simple problems in subtraction and division. The author says: "As we struggle with the intricacies of this computation, our admiration of the mental power of Archimedes, 'The Reckoner,' increases by leaps and bounds."

The lay reader of this book will doubtless find it necessary to omit much of the mathematics in its pages and confine his reading to its narrative and descriptive portions; but, even so, he will certainly gain from it a better understanding of the events that he observes in nature, and will be drawn into closer rapport with the scientific age in which he lives.

PAUL R. HEYL

Washington, D. C.

Island Life in Lake Michigan. Robert T. Hatt, Josselyn Van Tyne, Laurence C. Stuart, Clifford H. Pope, and Arnold B. Grobman. xi + 179 pp. Illus. $4.00. Cranbrook Inst. of Science. Bloomfield Hills, Mich.

This is a concise and informative study of the mammals, birds, reptiles, and amphibians of the Lake Michigan islands lying off the northwest shores of the Lower Peninsula. Following brief accounts of the recent geological and cultural history of the region is an extensive annotated list of the vertebrates (other than fishes). Short but provocative chapters on modifications of habits and on factors of distribution as shown by the island fauna will be of particular interest to ecologists. An appendix tabulates the species found on each island. There are a good bibliography and index.

LORUS J. and MARGERY J. MILNE

Department of Zoology
University of New Hampshire

The Story of John Hope. Ridgely Torrence. 398 pp. Portrait. $5.00. Macmillan. New York.

All Our Years. The Autobiography of Robert Morss Lovett. x + 373 pp. $3.75. Viking. New York.

The science of education in America found two stout protagonists in John Hope (1868–1936), the fair, blue-eyed, blond Negro president of Morehouse College and Atlanta University, and Robert Morss Lovett (1870—), beloved adjunct of the University of Chicago. Their biographies, listed above, are among the bright spots of the 1948 book crop. Though wide apart in many ways, the careers of these near contemporaries have their parallels. Both were lifelong university men and inspiring teachers, yet both crowned their lives with extra-scholastic service—Hope in the field of Negro advancement and race relations, Lovett in the cause of liberalism and in the public service. Lovett had the fortune to be able to write his own life story, and he has done it superbly. John Hope did not live to write his own biography, but he has had remarkably good fortune too in the fact that so sympathetic and intelligent a person as the distinguished poet Ridgely Torrence undertook to do it for him. Both of these books attain a literary excellence noteworthy in this day of slapdash. Both deserve a wide reading by social and political scientists and educators.

PAUL H. OEHSER

Smithsonian Institution

International Rules of Botanical Nomenclature. Compiled from various sources by W. H. Camp, H. W. Rickett, and C. A. Weatherby. 120 pp. $3.50. Chronica Botanica. Waltham, Mass. Stechert-Hafner. New York.

This is a second printing, by the offset process, of this important compilation. The unofficial special limited edition (originally published in Brittonia **1**: 1–120. 1947) was prepared under the auspices of the American Society of Plant Taxonomists as a service to its members. The need was very great because, owing to war conditions, the original edition was no longer available. Now through its reproduction it becomes available to all investigators who may have need to consult it. Its issue in its present form is most timely in view of the active preparation of data to be considered at the Seventh International Botanical Congress to be held in Stockholm in 1950. This critical compilation is, in many respects, superior to the original Leipzig edition, notably in the complete and critically indexed list of conserved generic names. The addition of critical data in the form of footnotes is another innovation that is highly commendable, for these data tend to clarify certain entries, and call attention to various items needing further official consideration. This is a volume that should be on the desk of every working taxonomist.

E. D. MERRILL

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CORRESPONDENCE

RUPPIA BALLS

The circumstances which led to the publication in The Scientific Monthly of Dr. Essig's paper[1] on "The *Ruppia* balls of Little Borax Lake" had an unusual concomitant. The *Ruppia* balls were brought to the office of the University of California herbarium for identification and called to the attention of Miss Annetta Carter, senior herbarium botanist. Miss Carter, because of her wide botanical knowledge and experience, is often able to give valuable suggestions that can put the bewildered botanist or paleobotanist on the right track. In the search for seeds or other objects that might lead to their identification, the balls were shaken on a piece of white paper and the material examined under a microscope. In the material so obtained were many tiny, semitransparent, oval objects with one, or usually two, dark spots symmetrically located in the axis of the oval, somewhat after the manner of *Ailanthus* fruits.

The writer is completing a monograph of the fossil plants at Florissant, Colorado. Among the unsolved mysteries of the flora were the objects called *Ranunculus florissantensis* Cockerell.[2] It was seen early in the study of the flora that these fossils could not be those of *Ranunculus* seeds, but their true relations were a puzzle. The Florissant material is on thin, light-colored slabs of volcanic lake shales and consists of many hundreds of small brownish dots scattered over the surface of the bedding planes. These dots, when examined with a microscope, show an oval shape with a stemlike extension, in appearance like a thin membranous wing or semitransparent case, in which are embedded two (or, more rarely, one) round, seedlike objects. They resemble the seeds of members of the Bignoniaceae, but certain obvious differences render them distinct.

It happened that on the day the *Ruppia* balls were brought to the herbarium the writer also took his Florissant fossils to Miss Carter, hoping for a hint as to their identity. She examined them under a binocular microscope and at once recognized their correspondence to the small objects from the Borax Lake *Ruppia* balls. The fossils had reposed in the Colorado mountains for some twenty million years and very briefly in the museum of paleontology at Berkeley, waiting to meet their living counterparts in the University herbarium on one particular day in the summer of 1948—a coincidence almost beyond believing. Both the recent and fossil materials were brought to the attention of various specialists in entomology and botany, but no one recognized them. Finally, they were sent to the office of the California Fish and Game Commission in Berkeley, where they were identified as the egg cases, or ephippia, of the tiny, fresh-water crustaceae called water fleas (*Cladocera*), probably belonging to the genus *Moina*.[3] Thus *Ranunculus florissantensis* must be changed from the plant to the animal kingdom and the naming left to some competent biologist. The identification was due to the fortunate meeting of a slab of fossil shale from Florissant, Colorado, and a *Ruppia* ball from Little Borax Lake, California.

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3. Ward and Whipple. *Fresh Water Biology*. New York: Wiley, 1918. P. 705.

Professor E. O. Essig's article "The *Ruppia* Balls of Little Borax Lake" in the June issue of The Scientific Monthly was quite interesting in that it described a natural formation of fibers very similar to that encountered in a pilot-plant operation at Armour Research Foundation of Illinois Institute of Technology. In the pilot-plant operation, a dilute slurry, or suspension, of various fibers such as wool or cotton is agitated in a tank. It was found that under certain conditions of agitation the fibers instead of remaining evenly distributed in suspension would roll up into compact balls resembling very closely the *Ruppia* balls shown in Professor Essig's article. Such ball formation was undesirable in the pilot-plant operation, and considerable study was necessary before their elimination was accomplished. It was found that small localized eddies which moved the fibers about in a circular pattern started the formation of the balls. Once the formation started the balls rapidly increased in size by rotating and winding up additional fiber. Elimination of the ball formation was accomplished by changing the agitation so that small eddies were prevented. In view of the above laboratory duplication, we believe Professor Essig's conclusion that the *Ruppia* balls are formed by wave action is essentially correct.

Clark E. Thorp

Chemistry and Chemical Engineering Research
Armour Research Foundation

Fiber ball formations in the laboratory.